OCT 3 1927
Medical Lib.

Vol. XX., No. 10.] [August, 1927.

PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

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The Royal Society of Medicine.

President—Sir JAMES BERRY, F.R.C.S.

DISCUSSION ON BLOOD-LETTING.

W. G. SPENCER, M.S.

BLOOD-LETTING—ITS PAST AND PRESENT USE.

THE reason I put forward for making blood-letting the subject of a general discussion by the Society is that, whereas some physicians at hospitals are practising it so that students can learn its utility, in other instances, and particularly in private practice, there hangs over the procedure prejudices handed down from the general disuse of venesection, now nearly a century ago.

I shall quote recent examples of acutely urgent states which would have been speedily followed by death had not blood been let. Under circumstances venesection is the sole means of saving life. For more chronic conditions the ancients knew that extraction of a few ounces of blood served as an adjuvant to other remedies. To re-establish such indications further evidence is needed. Advocates have too often dwelt upon their successes. Unsuccessful cases must be described alongside the successful ones, if the latter are to carry weight.

The discovery of the circulation of the blood by Harvey exploded the basis upon which the ancients had founded their practice, viz., their theories as to the removal of plethora, that extraction of a portion of the *materies morbi* favoured dispersal of the remainder, and cooled the excess of innate heat, *phlegmone*, general and local inflammation. As they did not recognize the pulmonary circulation, they failed to understand the symptoms marking its impairment. The discovery of the circulation had no influence, however, upon the practice of blood-letting, which continued unaffected for the ensuing 300 years. Then within a decade it came to be discarded, particularly by hospital physicians, but it persisted much longer in private practice.

The reason for this rapid abrogation was the revolution in the art of diagnosis caused by systematic post-mortem examinations, and through discoveries by percussion, and auscultation, and by chemical examination of the urine. Hughes Bennett dared to say in Edinburgh that his predecessors, Cullen and Gregory, had been unable to diagnose internal inflammations. That dictum may be generalized and applied to all previous physicians and surgeons.

VENESECTION BY THE ANCIENTS.

Not only the procedure itself, but the theories concerning the efficacy of the loss of blood, had a prehistoric origin. The relief afforded by menstruation, by nose-bleeding, by hæmorrhage from piles and wounds, was quoted continually in support of the measure; indeed it has been conjectured that such natural occurrences suggested the undertaking originally.

2 Spencer: *Discussion on Blood-letting*

In the works of Hippocrates, blood-letting is treated as so well established as to need only casual references. The two aphorisms recommending venesection for ophthalmia caused blood-letting to be employed for the gonorrhœal conjunctivitis which prevailed after the Napoleonic wars, and it was continued after cataract operations up to von Graefe's time. Asclepiades, Themison, and their Sect of Methodici, recognized the secondary, adjuvant influences of bleeding which they connected with their twofold division of disease, *strictum* and *laxum*. It increased the activities of remedies and so promoted the state of *laxum*—sweating, expectoration, purgation, urination, sleep.

Celsus advocated moderation, the criterion being the patient's strength, the flow to be stopped as soon as it became brighter and thinner. Galen mixed good with harm; he measured the amount of blood extracted and stated the average; but on the other hand he boastfully recounted extreme amounts which had been let out.

Mediæval Phlebotomy.

The Arabian physicians extracted blood from a spot as far away as possible from the supposed seat of the disease, on the theory that a "revulsion" would be produced, and they opposed the opposite theory of "diversion," i.e., the withdrawal of blood from as near the seat of the disease as possible.

Henri de Mondeville's chapter, "Phlebotomia," affords a systematic description. The principle of "revulsion" *v.* "diversion" gave rise to violent controversies. The Paris School followed the Arabians, and expelled Brissot (1470-1522); his opinions were formally prohibited by Parliament, and the intervention of the Emperor and Pope sought. The College of Physicians in London, following the Italian School, held to "diversion," blood-letting from the neighbourhood of the disease.

The custom of bleeding, periodically, healthy persons was a mediæval one. Celsus, among precautions to be adopted for the preservation of health, whether by the robust or weakly, did not include venesection. Vegetius, however, in the fourth century, noted a custom of bleeding cattle in the spring, which had not gained general approval. Monks adopted venesection so frequently that *sanguinis minutio* for depletion, is often varied to *minutio monachi*. It was systematized in monasteries, and great churchmen, like Bede, wrote on it. Nuns followed suit; women were taught phlebotomy to avoid calling in men. St. Louis had to forbid the nuns at l'Hôtel Dieu de Pontoise to undergo blood-letting oftener than six times a year. Incidentally the custom of bleeding healthy people favoured the rise of the Guild of Barbers, and the sign over the barber's shop, the pole, the spiral round the pole, and the bowl advertised it. Seasonal blood-letting at spring and autumn, in addition to its use for disease, was an important item in the receipts of the general practitioner almost within present memory. As relics of astrology, almanacks and calendars noting favourable days continued to be published into last century.

Influential Opinions directed towards the Disuse of Blood-letting.

John Hunter, in his "Treatise on the Blood," put forward ideas revolutionary to traditional notions. The blood is a living fluid, just as alive as the solid tissues of the body. Surgeons let blood for injuries to the head although the Hippocratic Treatise on that subject had not mentioned the step.

S. D. Gross recorded a conversation between Hunter and Physick, then his resident pupil at St. George's Hospital. A man who had fallen from a scaffold had been brought in utterly unconscious. "What shall I do? Shall I bleed him?" asked Physick of Hunter. "Bleed him? Bleed him, sir? No, sir, you would kill him outright. Wait, sir, until he reacts and then bleed him."

Marshall Hall, beginning whilst still at Edinburgh, wrote on diagnosis, continuing the twofold division of disease. What Asclepiades had termed "strictum" and "laxum," Hoffmann "tonic" and "atonic," John Brown "sthenic" and "asthenic," Marshall Hall called "inflammatory" and "irritative." He continued for "inflammation" to recommend venesection. His "irritation" was produced by repeated extraction of blood, and the signs of it were perversely taken to indicate "inflammation" and to call for further blood-letting. Repeated venesections, whether in adults or in children, caused throbbing of the carotids and in the head, stabbing pains through the temples, mental wandering, fainting, delirium, which were attributed to meningitis. Panting, breathing and palpitations were held to be signs of inflammation in the lungs and heart. The constipation, flatulence and foul stools were presumed to show intestinal inflammations.

Marshall Hall's communications to the Medico-Chirurgical and Royal Societies were summarized in the article by him in the "Cyclopædia of Medicine." Ten years later, in 1844, Copland's "Medical Directory" omitted all mention of venesection.

In the Sydenham Society's edition of Sydenham's Works in 1844, edited by Greenhill, the references under "Venesectio" in the index occupy a column. Four years later the same Society published the translation by R. G. Latham, but in the index there is no reference whatever to the subject under any heading "Blood-letting," "Venesection," "Missio Sanguinis," etc.

P. Ch. A. Louis, in Paris, exerted more influence as a hospital physician and a teacher who based his diagnosis upon experiences gained from post-mortem examinations and by examination of the chest. Moreover he introduced the comparison of statistical summaries of cases by which he proved that blood-letting did not benefit pleuro-pneumonia, erysipelas of the face, or angina of the faûces. His writings put a stop to the furious blood-letting pursued by Bouillard and Broussais.

Hughes Bennett, after declaring that Cullen and Gregory had not been able to diagnose internal inflammations, published "A Collection of the Cases of Pneumonia treated in Hospitals," whether at home or abroad, by blood-letting and tartar emetic, in which a mortality of one in three had occurred. Of sixty-five cases under Bennett, placed on a careful diet only, sixty-two recovered and three died, a mortality of 1 in $21\frac{2}{3}$.

On country practice, Sir James Paget and Benjamin Ward Richardson gave their reminiscences. Paget, in his address at the Norwich meeting of the British Medical Association in 1874, related that during his apprenticeship he had seen blood let from healthy farmers, from women who believed that their pregnancy would not otherwise be passed through with safety, also from patients already anæmic. He had seen no harm done; none the less, he added: "I am far from suggesting that any good followed this method of procedure." Paget passed lightly over instances of "adhesive phlebitis," but besides injuries to the artery or nerve, many cases were published of fatal septicæmia and pyæmia directly due to the lancet. B. W. Richardson had watched his uncle and had himself further experience of venesection as a direct life-saving operation, in cases to be mentioned below.

Impairment of the Pulmonary Circulation—the Capital Indication for Venesection.

Subsequent to the discovery of the circulation, there were experiments on asphyxiation connected with blood-letting, by early Fellows of the Royal Society, by Malgaigne concerning suffocation from charcoal fumes, and by Schafer in 1916, who experimented with chlorine gas. The experiments on animals allowed of direct observations upon the hindrance to the pulmonary circulation. The early experiments were the most likely source of the remark interjected by Sydenham in his

chapter on pleuritis: "The morbific matter, when diverted by venesection, comes within my control, and the opening by the phlebotome serves instead of a tracheotomy." The observation that venesection does the work of the windpipe explains Dryden's translation of the passage in Virgil's "Georgics" concerning inflammation of the hoof in sheep.

> "The ready cure to cool the raging pain
> Is underneath the hoof to breathe a vein."

The Oxford Dictionary gives two further contemporary uses of the phrase "breathe a vein," which it notes as "archaic and obsolete." As a phrase that may be so, but its implication is a reality to-day.

Summary of Asphyxiation Experiments.

The following is a brief account of what has been observed in the course of asphyxial experiments. The venosity of the blood increases; this renders the capillary walls more porous to the fluid portion of the plasma, so that there is an exudation of of fluid into the surrounding connective tissues. Consequently the blood, in losing up to 10 per cent. of its fluid, becomes thicker, viscous, "like treacle," "tarry." Corresponding with the diminished percentage of fluid, red blood corpuscles are increased proportionally; their hæmoglobin is abnormally reduced, so that they have an increased tendency to take up oxygen. The result is that less and less of darkly venous, viscous blood arrives in the left auricle, and through the left ventricle and coronary arteries reaches the muscle of the heart, which is thereby more and more reduced in activity.

If now, even although natural respirations have ceased, whilst the heart is still beating, there is a letting out of blood so freely that the blood-pressure in the pulmonary circulation suddenly undergoes a fall, afferent impulses start in the lungs and are transmitted through the pneumogastric nerves to the respiratory centre, where inspiration is provoked. Granted that there is now a free entry of the inspired air, and it reaches the pulmonary capillaries, there occurs a re-oxygenation of the blood, favoured by the increased proportion of red blood corpuscles, and the excessively reduced hæmoglobin. An additional consequence of the sudden reduction of blood-pressure is that a rapid withdrawal of fluid from the surrounding connective tissues back into the blood takes place, causing the blood to become thinner, less viscous. Thus an increased quantity of oxygenated and thinner blood passes from the pulmonary capillaries through the pulmonary veins to arrive at the heart muscle by way of the coronary arteries. The heart muscle thereby regains strength and activity.

States of Grave Urgency threatening Death from Impairment of the Pulmonary Circulation in which Venesection saves Life.

(*A*) *Pulmonary and Cardiac Affections.*

In 1869, Henry Sutton, under the title "Venesection to Relieve Distension of the Right Side of the Heart, and Passive Congestion of the Lungs," described nine patients in whom blood-letting had been adopted as a last resource, after expectorants and other measures had failed. In all great relief was given; in some the relief was immediate during the flow; all the patients returned to the state in which they had been beforehand. For success it was necessary for the inspired air to reach the pulmonary capillaries, and this was prevented mechanically by inflammatory consolidation of the lung, and by mucopus in the bronchi.

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In 1890, Pye-Smith, at the Medico-Chirurgical Society, opened a discussion on "The Therapeutical Value of Venesection, its Indications and its Limits." Between 1875 and 1890 he had submitted forty-nine patients to venesection, and an abstract of each case is attached to his paper in the *Transactions*. By far the greater number of recent publications on venesection relate to the class of cases considered by Sutton and Pye-Smith. It is sufficient for me to refer to Clinical Lectures by De Havilland Hall at Westminster Hospital, and by Hale-White at Guy's Hospital.

The effect of venesection upon the overdistended heart has been observed *directly* by B. Gordon, who arranged patients with cardiac disease and a failing circulation so that X-ray photographs could be taken at frequent intervals. Cases of hydrothorax and ascites did not admit of this direct observation. The clinical improvement following venesection was accompanied by a diminution of the area occupied in the photographs by the heart. Blood-letting in the case of a moderately dilated heart was accompanied by a much more marked diminution of the cardiac outline than in the case of an extremely dilated heart.

Venesection for air embolism following upon the induction of an artificial pneumothorax.—Jessen has reported that between 1909 and 1923 there had been noted twenty-six deaths from air embolism during the setting up of a pneumothorax. The air embolism had happened, not only during the actual introduction of air into the pleura, but also after the withdrawal of the needle. Air from the pleural cavity may then be sucked into a vein of the lung which has been wounded when inserting the needle. Immediate venesection and the letting out of 300 to 400 c.c. of blood has been followed by a return of consciousness, speech and of the corneal reflex,—spasm being arrested. There should always be a preparation for instant venesection, whenever the induction of a pneumothorax is about to be undertaken.

Venesection for uræmic states following scarlatinal nephritis.—Adults suffering from uræmia, if relieved at all, are benefited only temporarily. The lives of children suffering from nephritis after scarlet fever, in which uræmic coma has threatened death, have not only recovered after venesection, but have been cured of nephritis.

Singer reported that of nineteen cases venesected, fifteen recovered. With these he compared nine earlier cases not venesected, of whom four recovered and five died.

Wilks accompanied a practitioner to a little boy suffering from bronchitis following whooping-cough, whom they found lying half-raised upon a pillow, gasping for breath, his face livid, the eyes starting out of his head, the external jugular vein being most apparent. The practitioner let several ounces of blood from the external jugular; the lividity passed away, the child sank back on the pillow in a tranquil sleep, and from that time speedily recovered.

Radial Arteriotomy in Young Children.—In young children suffering from so-called "suffocative catarrh" following broncho-pneumonia, capillary bronchitis, etc., it may be difficult to open a vein, whether the basilic, cephalic or external jugular. Baginsky and others have adopted radial arteriotomy, rather than the temporal arteriotomy used by the ancients. Eckstein and Noeggerath related four cases with three recoveries. A finger's breadth above the wrist, a 1-cm. incision is made over the artery; the artery is cut across, so that 2-3 oz. of blood, not more than 100 c.c., in the proportion of 5 c.c. per 1 kg. of body weight, is let out rapidly. The rapid loss of blood is essential to obtain the reaction. Each end of the artery is ligatured, the skin incision is closed; aseptic care is presumed; no anæsthetic is required; healing by first intention has followed.

Spencer: *Discussion on Blood-letting*

(*B*) *Apoplectiform Seizures.*

In grave apoplectic cases, how can cerebral hæmorrhage be recognized offhand?

I was called by my colleague, de Havilland Hall, to an American aged about sixty. We were told that he had been seen previously by the first physician of his day, who himself had published recommendations as to blood-letting. He had diagnosed cerebral hæmorrhage and prognosticated early death. Thereupon de Havilland Hall had been sent for. The patient was deeply cyanosed, his breathing from being stertorous was becoming periodic, his extremities cold. With the arm hanging down beside the bed, I squeezed out thick dark blood until a redder and thinner flow began. With that the cyanosis lessened, the breathing became deeper and regular, a pulse at the wrist could be felt. He soon recovered consciousness; there was no paralysis. He left for America at the end of a month.

Soon after a practitioner near the hospital summoned me to a very fat butcher. He seemed in much the same state as the case just mentioned. I could only squeeze out dark, thick blood; no spontaneous flow followed. He was dead in a few minutes; an extensive cerebral hæmorrhage was exposed at the post-mortem examination.

B. W. Richardson was called to an aged man who was profoundly comatose and frequently convulsed. Believing it to be a case of uræmic coma the effect of which the profession had come to fear, Richardson refused the request of the relations, who believed him to be suffering from a stroke. They thereupon sent for a neighbouring practitioner who acceded to the request to let blood. There followed complete recovery, and Richardson learned that the old gentleman had many a laugh at his expense.

Milne related a striking instance of threatened apoplexy in a very heavy drinker, with extensive arterial degeneration, and urine pointing to small cirrhotic kidneys. The man was in a state of marked excitability, the face engorged, eyes protruding, high pulse tension, the heart area enlarged downwards and to the left. He was irritable, confused, giddy, impressed by impending calamity, obsessed by the fate of ancestors and collaterals who had had hemiplegia. He was bled to 27 oz. A loss of memory for three weeks, which was recovered from completely, was held to indicate a small frontal hæmorrhage. He lived for two years and then died in coma, not having again been bled.

Wulston had previously examined a patient who presented numerous degenerations prodromal of cerebral hæmorrhage. He was later seen in a severe apoplectic seizure, from which he recovered after blood-letting. This incident was repeated five times at intervals, and the patient had recovered after the fifth seizure and venesection.

(*C*) *Status Epilepticus.*

A recent discussion in the Society indicates that a wide view is prevalent as to the origin of status epilepticus.

Wilks said that a strong agricultural labourer was admitted to Guy's Hospital, and he found him in a fit which had already lasted four hours; there were continual convulsive movements and an increasing lividity of the surface. Blood was let and flowed freely; the face became pale; the man opened his eyes and spoke for the first time since the commencement of the fit, the interval having been to him blank. Afterwards he had no fit for a long time.

A middle-aged woman in constant convulsive movements, which had been termed uræmic without confirmatory evidence, was frothing at the mouth and quite comatose, almost pulseless, the whole body livid, the extremities cold. She was bled into a small washing basin until the lividity passed off; the convulsions ceased, the body became warmer, and later in the day she conversed calmly. She was alive years afterwards.

Milne saw a girl, aged 17, in status epilepticus, who had been having a fit about once a week. She was unconscious, in a succession of fits, purple, sweating profusely, and the heart-beats had begun to be irregular in time and in quality. About 20 oz. of blood were let; she ceased twitching and squinting, sat up in bed, recognized her friends and asked what all the trouble was about. The next day she was well and buoyant; menstruation, which had been checked, finished normally. There was no recurrence of a fit for $3\frac{1}{2}$ months, although

bromides were withheld which previously had been given until she was covered with acne. Subsequently she was seen again in status epilepticus by Milne's successor in practice, who, differing in view, did not let blood; the girl died in the fit.

(*D*) *Heat Stroke.*

Richardson was looking on in his uncle's surgery when a powerful man was brought in from the reaping field, pale, speechless, unconscious and so rigid that he was forced into a chair with difficulty. His uncle at once let blood from both arms simultaneously. At first there was a feeble flow, then a full stream, until after about two pints had escaped the blood became bright red, the man's muscles relaxed, he breathed deeply and looked around. Seeing Richardson holding one of the bowls, he said "You're a good boy." The flow stopped, and the arms bandaged, the man got up, and walked away to his home. He did not return, but a few days later was met, driving a cart, sitting on the shaft, and smoking his pipe.

Perry related that a stoker, asleep in his bunk on a very hot night in the Indian Ocean, was found comatose, the rectal temperature 107° F., the skin pale and dry, the pupils widely dilated, the pulse small and feeble. He was at first treated by cold and iced water, brandy and strychnine, following upon which the heart became more feeble, the pulse smaller and irregular. He was then bled to 20 oz. Whilst the blood was still flowing the skin lost its lividity, and the pulse became fuller and stronger. Good recovery attended the further use of iced water douches and quinine.

Klein said that a ship's stoker, a very powerful, sober man, on the second day out from Germany, after several hours in the stoke-hole, complained of thirst, then suddenly became unconscious and lapsed into such violent convulsions as to require four men to hold him. He was brought up on deck into the breeze, bathed with iced water and artificial respiration continued. Under this his heart failed rapidly and cyanosis increased. Thereupon he was bled and after about 250 c.c. of blood had been let out the pulse and breathing improved, convulsions ceased, and he fell asleep. After ten hours he awoke sound and hungry, and eight days later resumed work.

In French literature there are reports on venesection for heat-stroke occurring on army manoeuvres and in the tropics. There was much heat-stroke during the war in Mesopotamia, and Willcox and others have written on blood-letting. Couldray said of himself that he was affected on the voyage to Basrah and had not been relieved by other measures until severe epistaxis occurred during sleep.

In this and other instances a distinction should be made between what can be carried out quickly at an organized hospital, and the venesection which admits of immediate application in the absence of other remedies.

(*E*) *Asphyxia by Non-Irritant Gases.*

Concerning the treatment of asphyxia by artificial respiration, J. S. Haldane, Henderson and Haggard have explained that artificial respiration, including the administration of oxygen, may fail to start natural respiration because apnœa is set up. Hence they recommended the addition of a percentage of carbon-dioxide gas to the oxygen in order to excite the respiratory centre by afferent impulses through the pneumogastric nerves. But, as stated above, a sudden reduction of blood tension also causes an afferent excitation of the respiratory centre. Hence may be explained numerous reports of asphyxiatiön by carbon-monoxide and carbon-dioxide gases, the instances of men who have been submerged in water, the cases of water-gas poisoning; in which, after artificial respiration and other remedies had failed, venesection started spontaneous breathing.

I mention a recent communication. Jaksch-Wartenhorst reported three cases of carbon-monoxide poisoning with severe symptoms treated by venesection, followed by saline infusion and cardiac stimulants; the patients recovered.

Methæmoglobin poisoning.—Hirsch described very fully the poisoning of a chemical student by fumes of phenyl hydroxylamine (C_6H_5NHOH) which caused the hæmoglobin in the blood to be changed to methæmoglobin. Immediate treatment in various ways was applied without success until 300 c.c. of blood was extracted by venesection. It was of a brown chocolate hue, quickly clotting. There was an immediate return of natural respiration and a rapid recovery.

Gordon-Watson reported the poisoning of six men caused by the explosion of ammonal in a dug-out. All were extricated, but severe symptoms supervened. J. S. Haldane formed the opinion that the men were first affected by carbon-monoxide, then by methæmoglobin poisoning. They became dark, ashen-grey in the face, and their mucous membranes black. Two of the cases were not benefited by oxygen. Extraction of a pint of tarry blood abolished restlessness and induced sleep. This was followed by transfusion of blood and infusion of saline fluid, and the men recovered from an otherwise hopeless state.

There have been many accounts, lately, of instances of poisoning by the carbon-monoxide gas from petrol engines in closed garages, in which artificial respiration has failed.

(*F*) *Venesection in Irritant Gas Poisoning.*

Hale-White described how he relieved by venesection a man who had inhaled nitric-oxide fumes. Ultimately the man recovered completely.

In South Africa venesection was used for men gassed by the fumes given off by dynamite. For the chlorine gas poisoning in April, 1916, at the front in France, Hebblethwaite noted thirty cases in which venesection was practised. Of ten cases, equally cyanosed and distressed, five were venesected and did well; in two not venesected the men died within twelve hours. Three men, after becoming more distressed and livid, were bled and afterwards required cardiac stimulants. Of thirty cases venesected, twenty-seven men were evacuated to the base by the end of a week, and twenty-five men were alive a year later.

Schafer experimented on dogs poisoned by chlorine gas and free blood-letting was found to be the only remedy which produced an immediately beneficial effect. Extensive experiments have also been carried out in the United States by Underhill and Goldschmidt.

(*G*) *Acute Poisoning treated by Blood-letting.*

Acute alcoholic poisoning may cause violent delirium followed by death. Chronic alcoholism attended by delirium tremens should be distinguished; even the ancients opposed the bleeding of drunkards. Duplant, by using delirium tremens in the title of his paper, rather prejudices his advocacy of venesection.

Two of his cases, after hard drinking, were operated upon under ether. The patients became violently delirious, and in grave danger, unrelieved by drugs. One blood-letting of 500 c.c. was followed by calm and recovery.

Violent delirium and death have been known to follow ether anæsthesia, where there had been no drinking of alcohol beforehand.

T. Oliver adopted blood-letting in an instance of *carbolic acid poisoning*.

Mushroom Poisoning.—In this country people mostly confine themselves to the *Agaricus campestris* from high wind-swept meadows and downs, and also to its cultivated variety. They avoid fungi growing in damp and boggy spots, in woods, under and upon trees. In France and other countries Azoulay, Duplant and others have stated that a number of deaths occur, 100-200 a year, even more during the war when food was short. The fungus most commonly mistaken for the edible mushroom is *Amanita pantherina*. The extreme cyanosis which quickly ensues can be relieved by free blood-letting. Recovery has followed.

CHRONIC AFFECTIONS RELIEVED BY BLOOD-LETTING.

I have already mentioned that in order to establish more firmly the indications for extracting blood in chronic affections there is need for statistical summaries of all cases, successful and unsuccessful. Small quantities of blood can be extracted conveniently through cannulæ, lubricated by an anti-coagulant. It has been well known from olden times that blood-letting serves as an adjuvant to drugs in promoting sweating, expectoration, purgation, urination, and sleep.

Pain caused by aneurysm, and headache the consequence of high arterial tension, can be relieved by bleeding as noted by Pye-Smith, H. French and others.

Polycythæmia of the type which results from over-production of red blood-corpuscles in the red marrow of bone is cited by Plehn as the one instance in which plethora really occurs. Relief follows venesection, but there is indicated, in addition, the X-raying of the bone-marrow in order to diminish its excessive activity.

Chlorosis.

There have been many experiments upon animals to show that after the first blood-letting there results an increased activity of the red bone marrow. The loss of red blood-corpuscles is rapidly made up, although the restitution of the hæmoglobin takes a much longer time. But upon repeating the extraction of blood the reaction in the red marrow weakens and fails.

Half a century ago the largest group among hospital medical out-patients were the young women and girls with chlorosis—among them were some obstinate and even fatal cases. In Germany, especially, whilst University professors looked askance at the practice, in general practice there was much use made of letting a few ounces of blood. A chief advocate was Dyes: 12,000 cases have been attributed to him. I recount a case he saw in 1849.

A girl of well-to-do parents had suffered from chlorosis for seven years, and had been under continued treatment by many doctors. For months she had kept to her bed, had become emaciated, her skin cold like a corpse so that no leech could be made to adhere her feet and eyelids œdematous, her breathing panting, her heart thumping; she had obstinate constipation, insomnia, anorexia and expressed a desire to die. Dyes had the limited objective of affording relief to the distress caused by the dyspnœa and palpitation. He could not find a vein at the elbow, and had to dissect down to the vena cephalica in the arm, from which 3 to 4 oz. of faintly red blood was let out. The next day there had been sleep, less panting and palpitation; the skin was warmer, and there was an appetite for food. From that day she improved, only needing aperients. Fifteen years later she reported that she was in good health, had married, and was the mother of children.

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Dr. HERBERT FRENCH, C.B.E.

I fully agree with all that has been said as to the great value of venesection for the long list of acute conditions of various kinds to which Mr. Spencer has referred. I propose to confine what I have to say to an advocacy of venesection for chronic conditions in people who are walking about, and particularly for two groups or types of people—one rare, the other common.

The rare type has already been mentioned by Mr. Spencer, namely, spleno-megalic polycythæmia. That is a malady which, I think, is not so uncommon as it is thought to be. I have seen such people treated in different ways at various times, but by no treatment have they benefited more than by bleeding. Having had it done once, they come back spontaneously for a repetition.

There is one case in particular, a man at St. Albans, a veterinary surgeon's assistant; he has been coming up four times a year during the past ten years to be bled, and he is in better health now than he was ten years ago. He is one of the typical cases, with an enlarged spleen, 8 to 9 million red corpuscles to the cm., and all that has been done for him in the way of treatment is recurrent bleeding.

The other cases I have had of it have done very well from it too. I have not seen a case of the kind do badly under it, and I know of no other treatment which is so good.

The other type of case is, as I have said, common, and it is difficult to define. It gets mixed up with the group of high blood-pressure cases, but such high pressure is not an essential part of the clinical picture, because some similar patients benefit from the bleeding just as much when their blood-pressure is not raised, and the bleeding does not help the patients *qua* reduction of blood-pressure. The benefit they derive does not depend on reduction of the blood-pressure, and cannot be measured instrumentally; it is indicated and measured by their feelings. The type I refer to occurs in both male and female, though the underlying cause is probably different in the two sexes. The males are frequently between 45 and 60 years of age, often very busy people, the over-worked, intellectual type, successful in business, probably taking too little exercise, and attending too many public dinners. The patient having been tremendously energetic in his work now begins to find that his brain will not work as well as it did; he finds that his head is "swimmy," and he complains of vague bilious symptoms. He finds that his heart is not working well either. He looks full-blooded, and he is a type which would benefit from being sent to a spa,—kept on less champagne and given more exercise. Those people, if they are bled, feel better almost whilst the bleeding is being done, and they are afterwards so much more alert and active at their work that they come back spontaneously to be bled again. It is a mistake not to give these people the benefit of what relieves them so much.

In women it is more often a state of affairs difficult to define; often associated with the menopause in some ill-defined way, a high blood-pressure which is not necessarily permanent; and feelings of great fullness in the head as though it were going to burst. When one has said to them, "You sound as if you would be better if you had blood let," they say they have often wondered why blood-letting has not been suggested before; their own feelings make them realize that if they had less blood in them they would probably be feeling better. They benefit emphatically from bleeding.

Why does bleeding help? It is not because it reduces the blood-pressure to a lower plane and keeps it there; for benefit accrues even when the blood-pressure remains unchanged. Physiologists teach that bleeding, especially for high pressure, is useless, because the patient, in a short time, replaces the fluid removed from the vessels by fluid absorbed from the tissues, and that fresh blood is formed within a fortnight or three weeks. May it not be for precisely this reason that venesection does good? By the removal of a pint of blood one is taking away about one-twelfth of what was in the body; if a new pint is made in its place in a few days one will have replaced one-twelfth of the old blood by new blood; this new blood, being young and fresh, serves, perhaps, as a rejuvenator of the whole. Anyhow, whether something of this sort is the explanation or not, there is no doubt about the re-invigorating effect of periodic venesection in certain types of ill-health in the middle-aged, and I wish to urge the more extended use of blood-letting for chronic as well as for acute conditions.

In the past, one reason why blood-letting has not been done has been that when carried out by the older methods it is apt to make such a mess. I remember well the first time I bled a patient in Guy's Hospital. The blood spurted in all directions and the sister had to get a new dress. This was when I used the old lancet method. Now, however, by means of the needle and bottle method one can easily bleed a patient to the extent of a pint, in about ten minutes, without spilling a drop of blood anywhere.

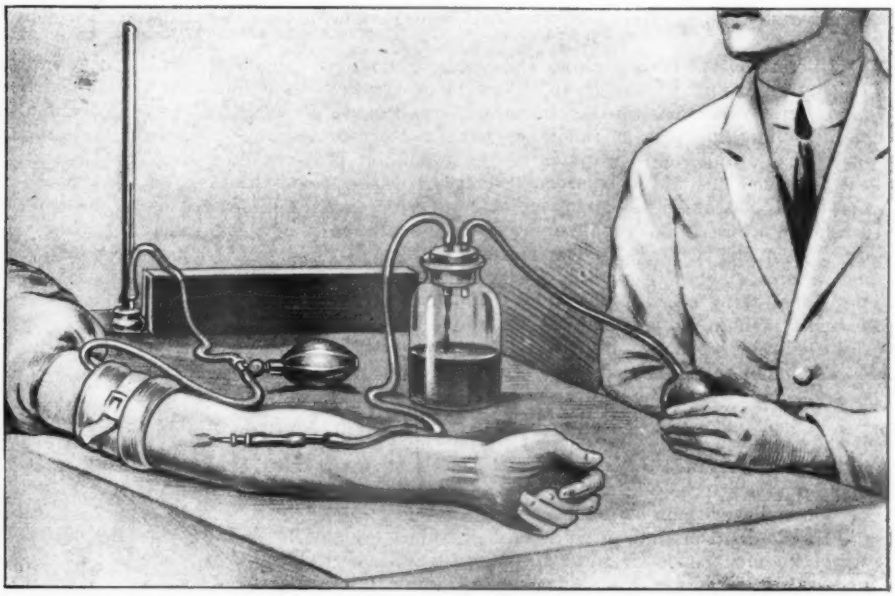

FIG. 1.—Venesection being performed by the needle and bottle method.

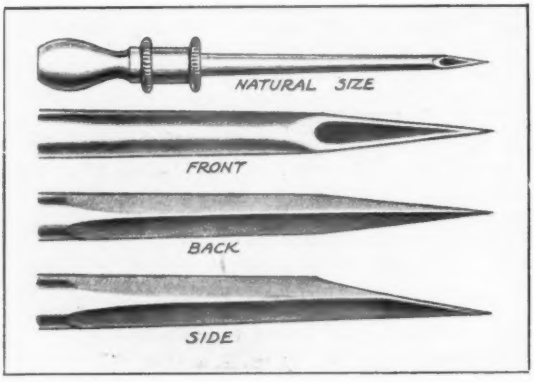

FIG. 2.—Venesection needle of elongated-cone type, in which clotting does not take place readily. The upper figure shows the actual size; the lower three figures are to illustrate the bayonet-shaped end, a device which reduces the transient pain of insertion to a minimum.

The simple apparatus needed consists of an ordinary glass jar with a cork, two short glass tubes which go into the cork, one long rubber tube which is directed towards the patient and has a glass window near its free end, and another rubber tube which has a hand-pump fitted with valves for exhausting the air from the jar. The patient sits at the table with the sphygmomanometer bag round the upper arm to distend the veins, the pressure in the bag being about 100 mm. Hg; one spikes the needle into the big vein in the anticubital fossa, and on using the hand-pump to exhaust the bottle the blood runs freely into the latter until it is full. Formerly with the use of a cylindrical needle one was troubled by clotting within it; now under the use of a special needle, see (fig. 2), there is no clotting, apparently because the blood as it flows up the needle is passing all the time through an increasing bore. The blood which is in contact with the side of the needle at the point is no longer in contact with it as it flows further up. Another feature of the needle is that it is sharpened along the back as well as round the oval opening, so that its free end is triangular in section; by reason of this it pierces the skin with far greater ease than does an ordinary antitoxin needle and hurts the patient less. No dressing is required for the punctured hole.

I hope that many will make use of this needle-and-bottle method of venesection because it is so easy, and because the objections to blood-letting on the score of mess have now disappeared.

[Dr. French also exhibited a number of pewter vessels which had been used by general practitioners for bleeding in the past, some dating from Stuart times, the most recent from the reign of George IV.]

April 7, 1927.

Dr. Fortescue Fox.

Venesection in Hyperpiesia and Neuro-vascular Hypertension.

The value of venesection was first brought home to me long ago in a case of thoracic aneurysm at the London Hospital. The man had distressing dyspnoea and pain, for which as an ultimate resource I made a small venesection, with immediate and spectacular relief. Two years later, when house physician, I had many similar experiences.

One was the case of a woman, of 29, with a mediastinal tumour, supposed to be malignant, but which proved to be an aneurysm. During the year she lay in the hospital repeated small bleedings gave her the greatest comfort. On one occasion the pulse sank in a minute or two from 138 to 108, showing the tranquillizing effect of venesection in some conditions of circulatory excitement. To this point I shall again refer.

Another case of aneurysm, subclavian and probably congenital, may be worth recording. A nurse, aged 34, after a time of much strain, suffered from a swelling, which, though scarcely noticed before, now rose into the neck with much pulsation and general distress. Mr. (now Sir Charles) Ballance and I both feared imminent rupture. The patient had already been confined to bed and ice-bags had been applied without marked improvement. Venesection, to about 10 oz., on three or four occasions at once allayed the local and general distress. The aneurysm slowly contracted, and she is now, 18 years after, quite well.

I have many records dating back to my hospital days, when we had no means of estimating blood-pressure and employed venesection at the London for obvious emergencies of the circulation. For threatening asphyxia and cyanosis in heart and

pulmonary cases, thoracic tumours, carbonic acid and other poisoning, when blood had accumulated in the veins, opening a vein not only relieved the cyanosis but appeared reflexly to relax the arteries. We also used venesection for some cases of fulminating pneumonia, [1] in acute pericarditis, in relief of the pain of pleurisy and in many cases of uræmic convulsions. As a recent example of the efficacy of venesection in acute toxæmia I quote a letter from a man who bled himself.

A woodcutter, aged 62, gashed his thumb with a saw. Fourteen days later lockjaw set set in and he gagged himself with a clothes peg. The gums and tongue turning black, he cleaned and sharpened his penknife and incised both gums and tongue in several places, from which he says "black blood" came freely. "The more I bled the easier I got," so he encouraged the flow by sponging the head and neck with hot water. After a time he remarked that the dark and clotted blood gave place to blood of a natural colour which flowed easily. "Of course," he says, "the nerves are in an agitated state and it is impossible to hold yourself still." After a profuse bleeding he slept for four hours, and next day returned to work.

Many equally remarkable cases are from time to time put on record, showing the immediate relief obtained from venesection (i) in circulatory emergencies due to many causes such as gas poisoning, pulmonary œdema, gun-shot wounds of the chest ; (ii) in embarrassment of the cerebral circulation, as in some cases of fractured base of the skull, and the status epilepticus ; (iii) sometimes at the onset of acute disease. A great many good observers have been impressed by its "obvious utility" [2] in such emergencies, and some, employing small venesections, have expressed surprise that so great an effect was produced by a trivial diminution in the volume of the circulating blood. The mechanics of the circulation could not have been altered thereby ; surely therefore some other explanation was needed.

Some emergencies, however, require large venesections.

A case in point is that of the "sunstroke" of Dr. Weir Mitchell. Dutton [3] describes how he over-heated himself in walking on a hot day in August, came home, became unconscious and so remained for ten days, finally recovering consciousness after prolonged mustard foot-baths. Thereupon Weir Mitchell insisted upon being bled, and between 25 and 30 oz. were taken. A good recovery ensued.

The performance of venesection in acute diseases, especially those with a sudden and dangerous onset, and in the various circulatory emergencies, surgical as well as medical, properly belongs to general and hospital practice. With the exception of apoplexy, my own experience in this category of cases is very limited.

But I submit that venesection has another field of equal or even greater usefulness. For many years I was occupied at one of the mineral water health resorts in the study and treatment of the incipient and chronic disturbances of health that are met with in middle and later life. The health seekers at these places are not as a rule actually ill, but, as it has been well said, they are "candidates for illness." It appeared to me that these cases had certain characters in common, and that a prominent feature attaching to most of them was disturbance of the circulation.

Having exchanged the hospital for the spa, it was not long before I had another dramatic proof of the value of venesection.

A lady nearly 70 years of age, of a stout and plethoric habit, and a *bon vivante* came to Strathpeffer and drank freely of the waters without advice. She considered herself very well, except that her head and her breathing had been troubling her for some months. The sulphur waters, which are not purgative, increased the flushing and the headache. After a few days, one afternoon, while sitting on the sofa, she lost her power of speech, became

confused, dropped her book and fell back unconscious. I saw her a few minutes later and found the unconsciousness deepening into profound coma. The face was purple, the pupils fixed and dilated, the limbs in restless convulsive movement, the right cheek flapping in respiration—obviously a case of left cerebral hæmorrhage. I bled immediately from the left arm. The force with which the blood was emitted was extraordinary. In a few moments the convulsive movements ceased and the pulse sank in frequency and force. I determined to go on as long as blood would flow and in the end took 35 oz., leaving her with an ice-bag on the left side of the head. In eight hours she regained consciousness, and on recovery declared that she was better than she had been for years. The right leg and arm were weak for some months and she was liable to confusion of speech and some want of memory for words, when taxed in conversation. For several years after, leading a guarded life, she remained quite well.

Not a few cases of apoplexy have occurred at the spas, and ought, I think, to be recorded. The sudden ingestion of water is sometimes in excess of elimination and fluid passes into an already embarrassed circulation. And, since the function of elimination by the kidneys, bowel and skin is very commonly defective, the danger is by no means an imaginary one. It is perhaps surprising that so few accidents occur. In a very early case book I find this note:—

"A stout plethoric woman of 50, complaining of headache, giddiness and flatulency, drank the waters and was dieted. Three months later she fell unconscious and died in two hours. In such another case can we recognize the danger of apoplexy and avoid it?"

A certain proportion of the invalids who go to the spas—and not only the obviously hyperpietic subjects—are not in a condition for taking the waters without preliminary treatment. In many cases I have advised a preparatory venesection or a course of purgatives.

In a series of papers [4] extending back to 1888 I have put on record a number of cases, showing the immediate effects of blood-letting, and also the later results, in histories extending over many years. It is the object of this contribution to define a little more precisely than has hitherto been done the indications for venesection in the chronic diseases of middle and later life.

Most physicians and all spa practitioners of large experience will recognize two different types of hyperpiesis; the ordinary well fed, jovial and florid, plethoric individual, and another more serious type, usually represented by a man with pale skin and anxious expression. In both of them the blood-pressure may be nearly up to the safety limit, but there is more risk for the pale subject owing to the contraction of his superficial arteries. Another familiar experience is the special incidence of spontaneous hæmorrhage in both the florid and pale hyperpietic. It is noticeable that menorrhagia, epistaxis, hæmorrhage or profuse watery evacuations from the bowel occur in many cases, and that after these occurrences headache, vertigo, depression, heat, flushing, palpitation and breathlessness are relieved. One is tempted to wish that nature's method could be relied upon. Unfortunately, among persons so affected, for one with a safety-valve there appear to be many without it.

Hyperpiesis and arterial and nervous hypertension are assuredly in the majority of cases the passing disorders of an epoch. They belong to a certain term of years, in which, in women, the catamenia cease, and, in men, the strain of modern life is at its height. I have myself watched so many subjects pass through the danger zone that I cannot believe that their high pressures are ordinarily or essentially connected with arterio-sclerosis or with any other structural disease. The behaviour of the vessels, especially after venesection and sedative bathing, appears to afford conclusive

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evidence that the essential factor is in truth a hypertension—a disordered nervous and circulatory function.

In some cases venesection causes an immediate fall of blood-pressure; in others, although the symptoms are relieved the pressure remains much the same.

A friend first came under my care when she was aged 48, for vertigo, sensations of heat and pain in many joints. At 54 the catamenia ceased and she had slight attacks of angina pectoris, the arteries being obviously contracted; at 57 there was a hæmorrhage from the rectum; at 58 a transient right hemiparesis and aphasia occurred. Vertigo, faintness and dyspnœa recurred, with a dry, chilly inactive skin; these troubles were relieved by wintering in Egypt. The next year, when she was in Scotland, the heart was hypertrophied, the peripheral vessels being contracted, only the head being hot and the face flushed, the extremities habitually cold, the systolic blood-pressure 160. The late Mr. Marcus Gunn reported that the retinal vessels were very small and "obviously sclerosed". In my view the disorder was mainly one of vasomotor tone, imposing a heavy burden on the heart and threatening the cerebral circulation. Venesection was carried out and repeated afterwards twice a year for six years; on one occasion, when the congestion in the head was severe, 24 oz. of blood were taken. Marked and immediate relief followed every bleeding. When the patient was last seen, at 69 years of age, there had been no return of the paralytic symptoms, her health was fairly good and the systolic pressure ranged from 140 to 160.

In many women with high blood-pressure a course of sedative baths after the menopause is often beneficial and may be recommended in preference to a venesection.

A spare and active woman, aged 48, whose parents suffered from gout and arthritis, herself had an attack of rheumatic fever when she was 29. From the age of 45 onwards a uterine polypus caused frequent hæmorrhages, and she complained of precordial pains during the colder months. When she was 50 the polypus was removed and hæmorrhage ceased. Then followed a sense of heat, flushing, more precordial pain, depression, restlessness and neuritis. At the age of 51 she underwent treatment at Aix-les-Bains; this relieved the tension, as also, in the following year, a course of thirty prolonged baths at Loèche-les-Bains, amounting to 130 hours in all in the warm water: Two years later pressure symptoms were relieved by venesection to 12 oz. After this they never returned, and at 62 years of age the pulse was soft and the tension normal.

It is unnecessary to multiply instances. For twenty-three years I watched the case of an old friend, whom I had to bleed during the onset of four or five pressure waves. Three of her relations had died from apoplexy in the early 'sixties, and venesection certainly helped her over this period. When she was 76 she had another access of high pressure, with an attack of glaucoma. I bled to the amount of 13 oz. before Sir William Lister performed the iridectomy, the circulatory excitement being allayed and the blood-pressure falling from 98/170 to 79/138. No further pressure symptom occurred before her death at the age of 81. (Chart I.)

Death from cerebral hæmorrhage in the critical years of the fifties and early sixties appears to be increasingly common amongst brain workers and public men, with a heavy incidence in certain families. Dutton tells us that the mortality from apoplexy is steadily rising in America, and remarks that "but for the revival of vitality in the population by peasant blood the true American would be extinct in 100 years." I have no doubt that this premature loss of valuable lives at the height of their activities could be often prevented by timely venesection when the pressures begin to rise. Further, it is clear that, as in the case already quoted, lives can sometimes be saved by venesection if carried out immediately after the hæmorrhage has taken place. Occasionally there are cases in which the rise of pressure seems to be uncontrollable, and one cannot hope for success. But I suggest that many failures occur because our preventive measures are not bold enough and are taken too late. My present practice in the presence of a dangerous pressure wave is to combine

a substantial periodical venesection with fasting in bed and a course of sulphates. A week's depletion course on these lines can be taken if necessary every two or three months.

An instructive example of high pressure that was perhaps impossible to control permanently is afforded by the case of a man aged 49, engaged in strenuous county and public work, and recommended to me by Dr. Wingate. Two members of his immediate

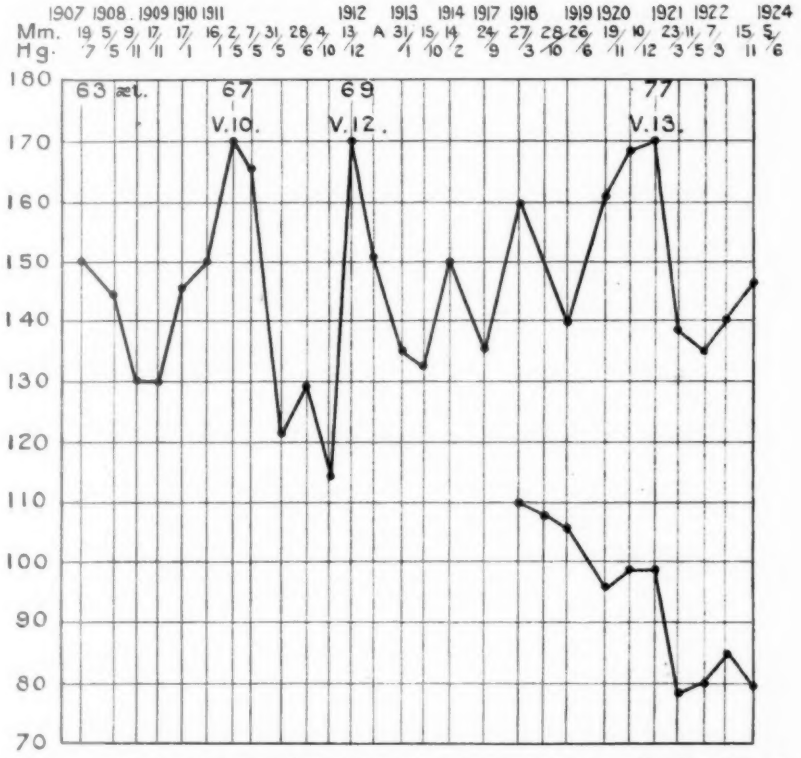

CHART I.—Female, at 58, precordial pain, breathless, "poor circulation." Family history of apoplexy. Effervescent baths, several years. At 64, restless, sense of heat, tinnitus; Norway, N.E. Scotland. At 65, "blood to head," Royat 2 years. At 66, slight anginiform attacks, attacks of profuse diarrhœa. At 67, apprehension, "pressure in head"—venesection. At 68, another pressure wave—venesection. At 69, Schlangenbad, fifteen baths. At 72 to 74, courses of effervescent baths. At 76 to 77, another pressure wave, glaucoma—venesection. At 77 to 82, period of falling pressures. (**A** = observation after venesection.)

family had died from angina and apoplexy. For twelve months he had had recurren epistaxis. He was pale, anxious looking, and rather breathless; his hands were cold; the heart's apex 2 in. outside the nipple; the sounds clear and ringing; the systolic blood-pressure 230 to 240; the temporal arteries were tortuous and throbbing, but the vessels were soft and there was no arcus senilis; the urine contained a trace of albumin. He had taken calomel, nitrites, iodides, special diet and had rested with little result. I bled him at the

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first interview, February 11, to 19 oz., systolic pressure falling from 240 to 200. On the 12th it was 170, on the 15th 160 to 170. In the summer he motored abroad, and took a course of sedative baths at Martigny (95° F., 55 minutes). On his return his pressure was 130 to 140, the heart but little accentuated. In the following February I found him working easily; the apex was below the nipple and the pressure 150 to 160. This summer he took a second sedative course at Martigny, every day remaining in bed without food until noon, drinking

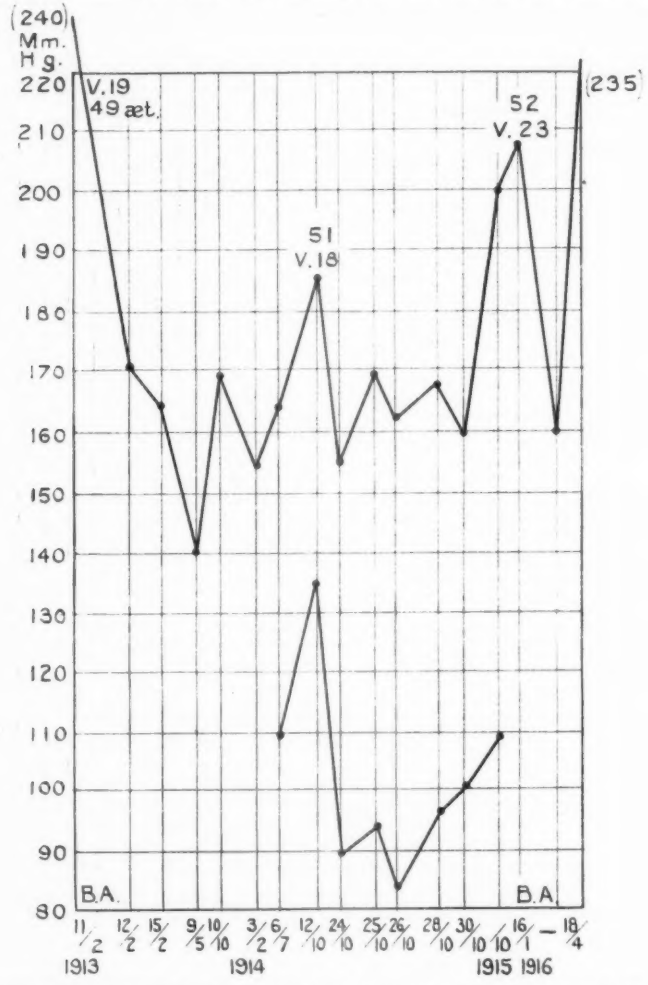

CHART II.—Male, at 49, headache, anxiety, pallor, cold extremities, epistaxis 12 months, heart's apex 2 in. outside nipple line—venesection; remarkable change in calibre of superficial arteries, sedative effect of baths (Martigny). At 50, working easily and well at public duty, heart's apex within nipple line. At 51, a second venesection. At 52, another pressure wave, heart again greatly dilated—third venesection, returned to arduous military work, and, at 54, died from cerebral hæmorrhage. (**B** = before, **A** = after.)

1,800 grm. of the diuretic water, and passing 1,900 grm. by the kidneys, and taking fifteen baths in three weeks. Dr. Huillier reported blood-pressures on arrival 120 to 210, on departure 80 to 155. In October—the war having broken out—he was working hard as a musketry instructor and the pressure wave rose. A second venesection, to 18 oz., produced no immediate change in systolic pressure (180), but on the five following days the readings were: 90/155, 95/170, 84/162, 96/168, 100/160. A year later, under the stress of work, the pressures mounted to 110/200, the apex was again 2 in. out. An X-ray photograph showed the heart's position to be nearly horizontal, with much enlargement of the left side, and some atheroma of the aorta. Three months later a third venesection reduced the pressure to 160, but he continued in full work, the pressure rose again in the summer to 220, and he died from cerebral hæmorrhage in the autumn, nearly four years after the first venesection. (Chart II.)

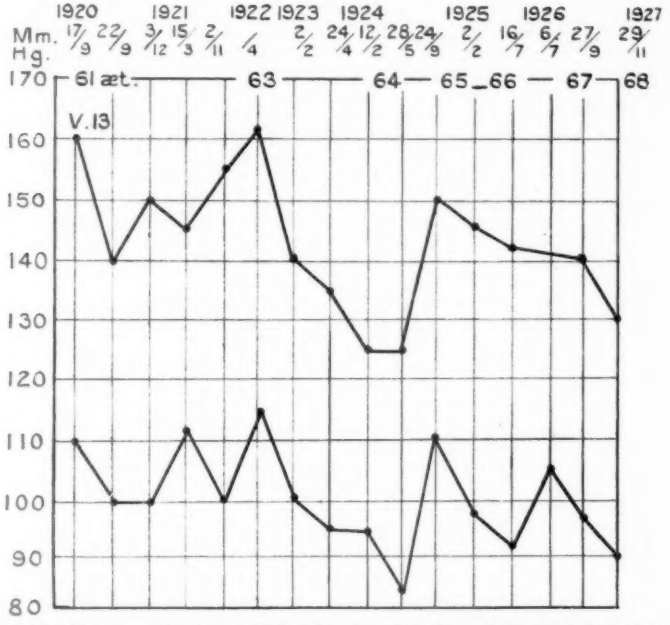

CHART III.—Female, at 61, "internal fire," restless, breathless (bad circulation all her life), heart 6 in. transverse measurement. Diet, KI, trinitrin, no effect—venesection, 13 oz. At 62, twelve effervescent baths, Bordighera. At 63, pressure wave. At 64, period of low pressure, failing ventricle, rest, tonics. At 65 to 66, another pressure wave, hæmorrhage from throat. At 67, heart 5 in. tr. At 68, period of falling pressures.

In this and other cases in which the arterial pressure has greatly exceeded the normal for months together, I have noticed a curious change in the character of the pulse, twelve or twenty-four, or possibly thirty-six hours after venesection. In the case just described the patient, who was a man of keen observation, called my attention to pulsation in the brachial, radial and ulnar arteries, which he had never seen before. Such a phenomenon can only be explained by supposing a widespread and habitual arterial contraction (hypertonus), which in these cases, after venesection, gives place in a few hours to arterial relaxation. It may be recalled that Sir William Hale-White has said that a small pulse is rather an indication for bleeding, a large dicrotic pulse a contra-indication.

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I have employed this method of treatment in a number of cases of more or less advanced cardio-vascular degeneration, with high arterial pressures. I have done so because I could not believe that high arterial pressure was beneficial, or a desirable compensation for increased peripheral resistance. Moreover, it seemed to me less reasonable to attribute this pressure to degenerative changes than to exaggerated tonus, which, as we have just seen, it is possible to relieve, so that—after a long period of contraction—the vessels, in fact, resume their natural size or may even become unduly dilated. In many cases of atheroma of the aorta, as in angina pectoris and aneurysm, the blood-pressure is liable to periodical and dangerous increase.

One more example may be given.

A lady aged 61 was referred to me by Dr. Hugo. She complained of flushing and discomfort in her head, restlessness and an "inward fire," systolic pressure 180/205. The X-ray photographs showed a greatly enlarged heart (transverse measurement, 6 in.) and atheroma and dilatation of the aorta. I bled to 13 oz., the pressure falling from 115/172 to 98/138 in four days. No further venesection has been necessary. At the present time, seven years later, the pressures have passed to a lower range, 90/130, and the transverse measurement of the heart is 4½ in. With due care as to rest and elimination, the patient follows her normal life. (Chart III.)

The same may be said of a spare man, now aged 64, with advanced atheroma and hypertrophy of the heart and occasional albuminuria. He has had hæmorrhages in the retina, from the pharynx and from the bowel. He was bled first at the age of 53, and during three or four waves of high pressure since. By careful avoidance of chill and wintering in the west of England he "carries on well." (Chart IV.)

In one case I even ventured to bleed a man, aged 64, in much distress from heat, tinnitus and throbbing in the head, although he had aortic regurgitation. I took 11 oz. of blood and the result was a happy one, but I am far from recommending venesection in aortic regurgitation.

No little discrimination is needed in the selection of cases for venesection. My conviction of its essential value is due perhaps to a hesitancy in adopting it. I have often been sorry I have not used it, but cannot recall one case in which I regretted that I had used it.

There is yet another kind of chronic disturbance of the circulation in which venesection has given good results in my hands. I refer to patients in the forties and fifties who complain of flushing, heat and throbbing in the head, increased by changes of posture but without any serious elevation of blood-pressure. Having observed that the circulatory excitement of aneurysms and apoplexy was allayed by venesection, I have employed small bleedings in these cases also.

I have now had a hundred cases of venesection, and my own experience has convinced me that in this procedure we possess a weapon for the reduction of high arterial pressure in a large proportion of cases. And yet in recent literature the beneficial effect is generally denied. Clifford Allbutt, for instance, in his "Diseases of the Arteries" in 1915 recommends it, but, in his latest writings on arterial sclerosis, he withdraws his commendation. Two French writers [6], in an otherwise admirable account of venesection, have recently stated that it acts entirely by its effect upon the venous circulation, and that medical men ought to know that opening a vein cannot assist the arterial circulation.

I think that the accepted doctrine, in so far as it has been founded upon physiological experiment, is somewhat misleading. It is true that Marshall Hall, found that no reduction of arterial pressure was effected in animals until about one-third of the total volume of blood had been removed. Later experiments have shown that it is necessary to remove one-tenth of the circulating blood in order to obtain the

minimum reduction of pressure, which only continues for two minutes. The effect of venesection in a case of high arterial tension or cardio-vascular disturbance is not the same as in a healthy person with normal circulatory equilibrium with all its regulating mechanisms and safeguards intact. We know, for example, that venesection usually increases the frequency of the pulse in normal people, but slows the pulse in heart disease, increasing its force and amplitude. It diminishes diuresis

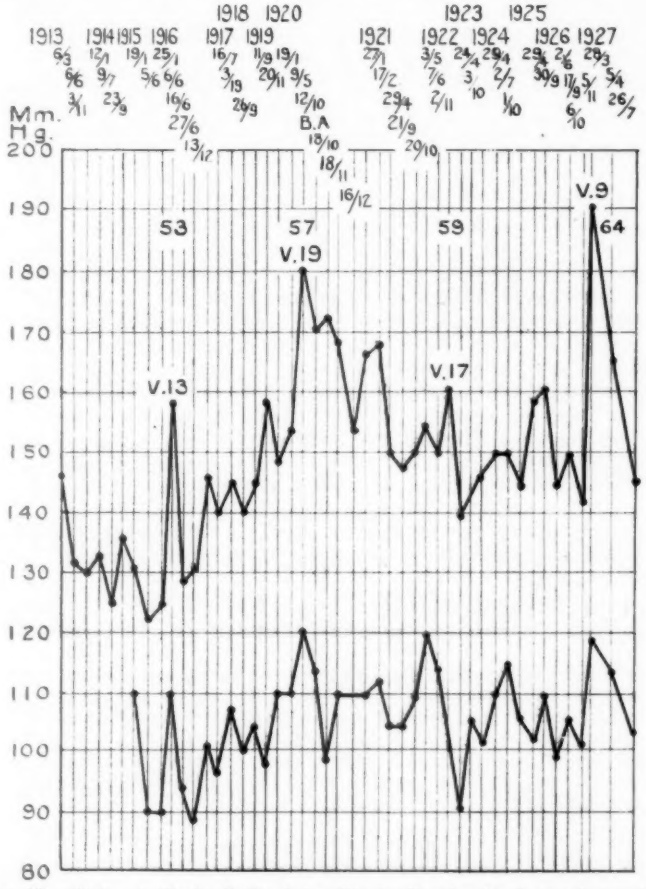

CHART IV.—Male, at 50, headache, intermittent albuminuria, hæmorrhage right eye, Evian-les-Bains, twenty-one baths. At 53, pressure wave, double aortic murmur, apex 1 in. outside nipple line, atheroma—venesection, winter in the West of England. At 57, another pressure wave, small hæmorrhage in eye—venesection, ten effervescent baths, apex within nipple line. At 59, slight wave—venesection. At 64, another wave, ventricle much dilated—venesection.

in normal people, but increases it and may produce a very crisis of polyuria in the presence of heart disease. It is possible to be misled by physiological analogies.

I would suggest: (1) that arterial hypertension with or without cardio-vascular disease is not an established equilibrium, but that it shows remarkable variations;

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(2) that it has heavy and dangerous exacerbations in many individuals at certain periods of life ; (3) that waves of high pressure often occur repeatedly during a certain period of years ; and (4) that venesection, when timed at or near the beginning of the pressure wave, effectually lowers the blood-pressure in a large proportion of cases and so prevents cerebral hæmorrhage and other accidents which are liable to occur.

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Sir William Hale-White, K.B.E., M.D.

If we had met on this day eighty or ninety years ago, several hundred people would have been bled in London this afternoon, for it was the universal practice that everyone who looked after his health should be bled every spring and fall. I have heard Sir Samuel Wilks say that if you went to almost any hospital in London in the spring and fall, you could see the people lying on the floor, in rows, recovering from their periodical bleedings. The total number bled in England in any one year must have run into hundreds of thousands. Now it is a rare event. It is the most striking thing I know in the history of medicine, this abrupt change of treatment.

To show how it was believed in not so very long ago, a doctor, in 1840, was accused and taken into Court for malpraxis because he had not bled a patient with pneumonia. The question of venesection led to the most embittered controversy which has ever taken place in medicine ; if you wish to see what angry things doctors can say to one another, read accounts of the great blood-letting controversy between 1850 and 1860.

The man who began to put us right was Marshall Hall, who published, in 1830, a book on bleeding. Three of his cases show what our forefathers were doing.

A man who had the misfortune to break three ribs was taken to hospital, and at 8 a.m. he had 18 oz. of blood taken away, at noon he had another 18 oz. removed, at 8 p.m. the same quantity, and again at 8 the next morning, and so on until, in two days, he had 112 oz. taken away ! It is not surprising that they remarked at the autopsy that the lungs were pale. And there is another case, that of a woman who suffered from post-partum hemorrhage. She was bled many times, and finally died. A poor man had a blow on the back, no external wound. He was taken to hospital and was bled several times, and died.

And so I might go on the whole afternoon quoting cases.

It is remarkable that our forefathers did this for no particular reason that we can see now, except that it was the fashion. Sometimes there were accidents besides death from the loss of blood. If you are interested in this subject you will find that a number of people died of pyæmia from sepsis of the venesection wound.

But bleeding is an excellent mode of treatment as used nowadays. The cases, above all others, in which it is of undoubted benefit are those where the right side of the heart is overfilled. An interesting and valuable guide for bleeding lies in the answer to the question : Is the patient livid ? If he is, it follows that the right side of the heart is over-distended ; it is like a paralytic bladder. Until you empty it

directly by bleeding from a vein it cannot contract and the patient will do badly. If you do bleed, and so allow the heart to contract, you will be surprised to find how much better digitalis will act in cases in which, before you bled, it seemed useless to give it, as it appeared to do no good.

Our forefathers bled a great deal for pneumonia. Most pneumonia patients are not livid and they do not require bleeding. Apoplexy has been mentioned several times to-day. You do not want to bleed a patient further if he is already bleeding into his brain, so in cases of apoplexy you must choose for bleeding those in which the apoplexy has caused embarrassment of respiration so that there is lividity; then you may bleed.

With regard to status epilepticus, which is a very rare condition in ordinary hospital and general practice, there, again, it does immense good in some cases, but the cases which require it will be very few, because they must be cases in which epileptic convulsions predominate over the other symptoms of epilepsy so that the patient becomes livid. These will benefit by bleeding.

All the foregoing are comparatively simple instances of cases which are benefited by bleeding.

I have very little experience of sunstroke, but we have heard that blood-letting is said to be very beneficial in its treatment. Possibly other methods of relief, such as bringing the temperature down by cold packs, are more efficacious.

You read of recommendations to bleed in uræmia. In this I do not think bleeding does much permanent good unless the patient has convulsions. It is recommended because it is thought that by this means you get rid of some of the poison, but you do not get rid of much of that in the pint of blood that you take away.

It relieves the pain of aneurysm, as has been mentioned, and I agree with Dr. French about the good effect it has in polycythæmia; I know of nothing which does so much good in that disease. Polycythæmia is the only condition known to us in which the total volume of the blood is increased. As has been said, polycythæmia is an innocent tumour of the blood.

I shall not say much about high blood-pressure; there is much to be said on both sides of that question. But venesection often reduces it. The question is whether repeated bleedings do good. You have heard experiences related which show that they do confer benefit.

There are two little points which are worth considering in regard to bleeding. One is, always to have the patient sitting up; for the reason that if he is sitting up and he faints, you can bring him round when he is lying down. But if he faints when he is lying down, there is not much that you can do.

The second point is: be careful how you bleed the young, for they bear it badly. And the extremely old bear it badly too.

Mr. Spencer mentioned arteriotomy. I think that has gone out of fashion. Leeches and wet-cupping are comparatively inefficient remedies.

I have seen a fearful mess result from bleeding in the ordinary way, just as Dr. French has, and I have no doubt that his method of doing the operation is the better one.

Dr. J. F. Halls Dally.

It is believed that Podalirius (1134 B.C.) was the first to practise the art of blood-letting. He bled the Princess Syrna for concussion of the brain. At a later epoch Pythagoras (500 B.C.) regarded blood-letting as a useless waste of life. This

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opinion, however, was contrary to that of Galen and certain other of the Greek physicians to whom Mr. Walter Spencer has referred. And so the pendulum swung throughout the ages; contrary opinions prevailed until, during the first half of the nineteenth century, the practice of this method may be said to have reached its zenith. Not only were the sick bled, but periodical bleedings were considered necessary to keep the body in health. Almanacks contained a list of proper days and seasons, especially spring and autumn, for abstraction of blood. Physicians used blood-letting to heal inflammations, and surgeons to prevent them. Hughes Bennett tells the tale of a young man who met with a street accident, and, upon recovering consciousness, found the surgeon in the act of bleeding him. Blood was frequently drawn in such large amounts as to cause fainting, or even death.

At that era blood-letting was perhaps more generally required than it is to-day, since our forefathers tended to a more robust and vigorous habit of body, being themselves hearty eaters and drinkers of heavier meals and stronger beverages than are now in vogue, and taking considerably more out-of-door exercise. This type of patient, of which the fox-hunting squire was an example, was of sthenic or hypersthenic habitus with distinct leanings towards plethora, and suffered the loss of greater quantities of blood than our more asthenic moderns could readily bear.

By the year 1857 contemporary observers noted that bleeding for acute inflammations had largely been abandoned, and that whereas formerly it was the custom to bleed early in such conditions as pneumonia and other acute infections, and to continue with large and frequent bleedings, at the later date mentioned blood-letting had become infrequent. This change of practice was variously attributed to change in type of diseases, especially those of the febrile group, and to a more correct appreciation of the principles of sound pathology.

Blood-letting, nevertheless, is still of use in certain conditions. In refractory cases of hyperpiesis venesection constitutes an important addition to our armamentarium. Particularly is this the case in respect of relief of symptoms rather than of actual height of arterial pressure, for my own experience is that cerebral hæmorrhage not infrequently occurs in association with arterial pressures that are high rather than with those that are extremely high.

I agree with Dr. Fortescue Fox that, if the beneficial results obtained by blood-letting are to be more than temporary, a course of starvation and depletion during the next few days is requisite. Otherwise the relief is merely transitory. In crises, speedy and direct ease is afforded to an embarrassed circulation and dilated right or left ventricle by rapid withdrawal of 300 to 500 c.c., which causes a reduction of from 5 to 30 mm. arterial pressure. In pericarditis, when evidences of cardiac defeat in the shape of cyanosis, orthopnœa and small rapid pulse appear, withdrawal of 100 c.c. or more of blood is efficacious. In cyanosis due to asphyxia and in acute pulmonary œdema, immediate relief is often experienced by the patient. But I have seen little result in uræmia due to chronic kidney involvement, though in the acute and subacute conditions with convulsions more effect may be observed.

Sir James Dundas-Grant, K.B.E., M.D.

When, as a student at the New Town Dispensary in Edinburgh, I was attending a little boy with scarlet fever and had the mortification of seeing him die with uræmic convulsions and coma, I read in the *Edinburgh Medical Journal* that in such cases there was sometimes recovery after venesection. I then decided that I would not allow the patient in such a case to die in my hands without having the chance of recovery afforded by this proceeding.

The first opportunity occurred in the case of a little girl in East London, who was rapidly succumbing to scarlatinal uræmia. Unconsciousness was almost complete, the heart-beat and pulse scarcely perceptible and my proposal to bleed the patient was at once turned down. It was then suggested that another neighbouring practitioner of greater age and experience should be called in to consult with me over the case. He urged me not to deplete, as she was obviously dying in any case, and if I bled her they would say that I had killed her. Fortunately the parents gave me the option, and when an ounce or two of blood had escaped, the pulse returned, the apex beat was soon perceptible and the patient became conscious, eventually recovering completely. I learned the subsequent history of my case through a young lady in one of our great establishments hearing my wife giving her name when ordering a gown. She inquired whether it was that of the doctor who was formerly in East London. She stated that I had saved her life, and that she was now quite strong and hearty.

I was once called in to see a boy who had fallen into the dock and had been resuscitated by the river police. On his return home I found him struggling to breathe; he was almost purple, his veins standing out like cords and he was becoming delirious. Venesection relieved him at once and in a few days he was quite well.

In a case of puerperal eclampsia and uræmia the patient's condition seemed absolutely hopeless and I called in the late Dr. Herman to share the responsibility as to course of treatment. We bled the patient to the amount of well over 20 oz., and applied a wet pack. No change was apparent until after she had been given a drop of croton oil, when in about half an hour rumblings in the intestines became audible. She then began to exhibit signs of re-animation and consciousness gradually returned. Recovery was complete.

In a case of what appeared to be acute pnuemonia in a middle-aged man, I called in one of our most respected consulting physicians, who perhaps too readily accepted my diagnosis and endorsed my suggestion that we should give him the benefit of venesection. The case proved, however, to be one of acute tuberculosis.

Within the last few years I operated upon a pharyngeal pouch occurring in an elderly man. Some adrenalin had been injected along the proposed line of incision. After the operation, while the patient was still on the table, I found his pulse so tense and full that a hæmorrhage into the operation-wound or elsewhere seemed imminent. I drew off a few ounces of blood by venesection and the tension disappeared entirely.

I am strongly convinced that in therapeutics there is a place for venesection, though its limitations are obvious.

Fleet Surgeon W. E. Home, M.D., R.N.

said he had spent a good deal of time in reading the books of old doctors, and he would give an account of venesection as it appeared to Mackay, a student in Edinburgh in 1842-44, who became an Inspector-General in the Indian Medical Service, and was the father of George Mackay, the ophthalmic surgeon in Edinburgh. Mackay told the speaker that when he was a student in Edinburgh the ordinary disease one saw in the medical wards was fever, which, it was now known, was a mixture of typhus and typhoid. There were three physicians in the hospital whose practice he followed: Alison, who practised physic, Henderson, who was suspected of being a homœopathist, and Craigie, who was a skilful physician, and published a book on the practice of medicine. They had three different treatments respectively. Alison gave his patients wine and opium and did not bleed them; Henderson gave his patients absolutely nothing; Craigie bled his patients. The question was asked, what was the difference in the mortalities? the reply was, "none," but there was an immense difference in the length of the convalescence. Alison's patients recovered after a fairly quick convalescence, Henderson's with a long convalescence, Craigie's with a protracted and terrible convalescence.

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Sir WILLIAM WILLCOX, K.C.I.E., C.B., M.D.

said that the subject of venesection had always been one of great interest to him, as he was a disciple of Dr. David Lees—an ardent believer in venesection—whose house physician and pupil he had been for many years. Without discussing the history of venesection he would summarize his views on the subject. His view was that there were five classes of cases in which venesection was beneficial.

(1) *Dilatation of the Right Heart.*—Sir William Hale-White had referred to this class. Unfortunately, students and others did not attach importance to percussion of the right side of the heart. It was one of the teachings of David Lees that dullness in the fourth space to the right of the sternum was a great indication of the size of the right auricle; it certainly was a valuable guide. Then, as Sir William Hale-White said, there were the cases in which the right heart was dilated, such as mitral stenosis, and certain cases of pneumonia—not the influenzal pneumonia, but the old-fashioned pneumococcal pneumonia, in which, often, there was much cyanosis—also cases of asthma, and of capillary bronchitis; in these venesection was often of great value. (In these cases a venesection of from six to ten fluid ounces gave great relief; quantities over this should not be withdrawn, since in some cases the operation might have to be repeated.)

(2) *Convulsions.*—This constituted another group in which venesection was of the utmost value. Examples of this kind were seen in status epilepticus cases, in which bleeding almost always stopped the convulsions. With regard to uræmic convulsions, he differed somewhat from Sir William Hale-White, who said that bleeding was not of much value in uræmic convulsions because the poison re-accumulated and the symptoms recurred. He agreed with that statement if the condition referred to was chronic nephritis, but there were many conditions of uræmia in which the kidney condition was only temporary, and stoppage of the function of the cells was due to degenerative changes in the epithelium, the best example of which was eclampsia. Bleeding stopped the convulsions, and often the patient made a complete recovery.

He had had a patient with a history almost analogous to that which Sir James Dundas-Grant narrated, the subject of uræmia, who was bled over twenty years ago and was now perfectly well and had since had a family. It was an acute nephritic condition, with loss of function; it was not a case in which the kidney was damaged irretrievably. Also in the cases of trench nephritis which were seen in the war, patients recovered quickly from it on being bled. He had many times bled patients with uræmic convulsions, and they made a permanent recovery. In cases with convulsions a large venesection of about twenty fluid ounces was required.

(3) *Heat-stroke.*—In 1917, he saw in Mesopotamia an enormous amount of heat-stroke, in the treatment of which venesection was of the utmost value, especially when cyanosis and convulsions occurred. If it were a case of removal of toxins one would think that putting salines into the patient would be of additional value, but practice did not support that view, for if normal saline were afterwards given the convulsions often began again. He had read, before the Medical Society of London, a paper on the subject of heat-stroke, and in that contribution he gave an account of other methods of dealing with the condition.

(4) *Polycythæmia.*—In the treatment of this condition it had already been stated that venesection was a most important form of treatment; that statement needed repetition.

(5) *Medico-legal Cases.*—This group embraced cases in which patients were suffering from forms of asphyxia of a violent type, such as from drowning, and from

hanging. Often, in these cases, death resulted from paralysis of the right heart, and if the patient was in a condition of extreme asphyxia, rapid venesection would often relieve the right heart, and then the other methods of treatment would enable the patient to recover.

Dr. Herbert French and others had referred to the great value of venesection in high blood-pressure. He (the speaker) thought the perspective given by Dr. Fortescue Fox was admirable. There were certain cases in which the high pressure was reached in a sort of wave, and in those, timely venesection was of great value. There were cases with an inveterate type of blood-pressure, in which the pressure was brought down, but in a short time it went up again. And there was a group of cases, as described by Dr. French and Dr. Fortescue Fox, in which venesection was of the utmost value.

No doubt, venesection had been abused in days gone by, but at the present time venesection by bleeding, leeches, etc., was a form of treatment which, in certain cases, was of the utmost value, and it should be practised more frequently.

Dr. H. Letheby Tidy

said that one or two points seemed to have developed from this very interesting discussion, both as to when venesection should be done, and when it should not. What had struck him in the debate was that there were conditions in which everybody was agreed it should not be done.

With regard to the chronic cases, it seemed to be agreed that bleeding should not be carried out for chronic anæmia, nor for conditions of low blood-pressure, two conditions in relation to which blood-letting had been brought into discredit in the past. Anæmia all could rule out. Low blood-pressure occurred in elderly people and caused symptoms which might be difficult to distinguish from some states of high blood-pressure. No doubt those subjects were bled in the past, thrombosis resulted, and discredit was brought on the system.

Everyone seemed agreed on the main basis as to when bleeding should be done. In polycythæmia its benefit was indisputable. There was also agreement as to the chronic high blood-pressure status described by Dr. Herbert French, Sir William Hale-White and Dr. Fortescue Fox, though the condition was only doubtfully defined by the term "high blood-pressure."

In regard to the acute cases in which bleeding was indicated, these were covered by Sir William Hale-White's description of "lividity"; with lividity one could also group dilatation of the right heart.

As to uræmia, it seemed that here there were two groups: the acute cases in which Sir William Willcox upheld that bleeding should be done; and the chronic cases in which Sir William Hale-White considered that it should not be done.

There was also the question of sunstroke, of which Sir William Willcox had had large experience, in the relief of which bleeding was stated by him to be of definite value.

Those seemed to be the main points which had emerged in the discussion, and they were in accordance with his own experience and views.

Mr. Spencer (in reply)

said that in confirmation of what Sir William Willcox said as to healthy people with heat stroke and with carbon monoxide poisoning recovering rapidly after being bled, that applied to healthy people. From the surgeon's point of view, the best way in

which he could emphasize the importance of blood-letting was to call attention to the enormous amount of carbon monoxide poisoning going on in various ways at present, in combating which, apparently, only artificial respiration was tried. It might succeed, but Professor Haldane had given reasons explaining why artificial respiration might not start respiration. In such cases let people try blood-letting.

Dr. Fortescue Fox (in reply)

said that those who were interested in the question of bleeding for high blood-pressure might with advantage watch the behaviour of the superficial arteries. In properly selected cases there was an almost immediate expansion of the arterial bed shortly after venesection, and it was to that he mainly attributed the benefit observed.

Section for the Study of Disease in Children.

President—J. D. Rolleston, M.D.

The Section and its Work, 1908-1926.

PRESIDENT'S ADDRESS.

By J. D. Rolleston, M.D.

It was perhaps inevitable that after occupying the chair of the Section of the History of Medicine for two years I should choose as subject for my address a retrospect of the work of this Section, of which it has always been my ambition to be President.

The Society for the Study of Disease in Children, which was founded in 1900, was incorporated not without some reluctance and misgivings on the part of many of its members, in the Royal Society of Medicine in 1908, the first meeting being held on October 23 of that year.

The Royal Society of Medicine, at the time of the amalgamation, consisted of thirteen Sections which have now been increased to twenty-four by the absorption of old societies or creation of new Sections. During the eight years of its existence thirty-four of the original members of the Society for the Study of Disease in Children, resigned, and five died, but these losses were considerably overbalanced by new accessions, so that when it became merged in the Royal Society of Medicine its membership was 319.

The Section now numbers 287 members, of whom 166 live in London, 114 in the provinces and seven abroad. Thirteen are Members of the Section only without being Fellows of the Society. The Section thus ranks sixth in the number of its members among the twenty-four Sections of the Society, being preceded only by the Surgical, Clinical, Medical, Obstetrical, and Pathological, in the order named.

The number of members and visitors present at the meetings usually tends to be about the same. During the pre-war period, 1908-1914, the average attendance each year ranged within the narrow limits of 35·0 and 41·7 at each meeting. The average attendances during the war fell to 25·8 during the first year, 19·0 during the second year, and 17·8 during the third year, but rose to 23·1 during the last year. On the cessation of hostilities the attendances at once increased, the annual average during the seven succeeding years ranging from 33·7 to 51·8 at each meeting. I am indebted to Mr. G. R. Edwards, the Secretary of the Society, for a table of the average attendances of the twenty-four Sections during 1925-6, showing that this Section comes tenth in order with 41. During the war this Section, like all the others, was forced to diminish its activities. The number of meetings in the session was reduced from eight in 1913-14 to five in 1914-15 and four in 1915-16; no special discussions were held, the provincial meetings were suspended, and owing to the paper shortage the number of pages allotted to the sectional proceedings was considerably curtailed, reaching a minimum of sixty in the session of 1917-18.

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England, however, appears to have been the only belligerent country in which pædiatric meetings were held regularly throughout the war, although less frequently than in normal times. The Société de Pédiatrie, of Paris, had to suspend its activities for fifty-three months, viz., from June 4, 1914, to January 21, 1919, and I cannot find any record of regular meetings being held by German, Austrian, or Italian Pædiatric Societies during the war.

In the holding of provincial meetings, organized by the provincial secretary, and the arrangement of special discussions, the Section has followed the example of the old Society, but it is only within recent years that the excellent practice has been resumed of holding some of the meetings at children's hospitals. The first meeting of this kind was held at the Hospital for Sick Children, Great Ormond Street, on February 23, 1923, and since then the Section has accepted the hospitality of the Children's Hospital at Paddington Green (February 26, 1926), and a second invitation to Great Ormond Street (April 23, 1926). Meetings have also been held in the Children's Departments of Guy's Hospital (February 22, 1924) and St. Thomas's Hospital (November 28, 1924), which in the time of the old Society had not come into existence, as well as at the National Hospital for the Paralysed and Epileptic, Queen's Square (March 27, 1925). The choice of a place for a provincial meeting has been mainly determined by its being a university town such as Cambridge (1911 and 1925), Manchester (1920), Reading (1921), Oxford (1922), Bristol (1923), Liverpool (1924), or Birmingham (1926), or a seaside resort such as Portsmouth (1910), Folkestone (1912), or Bournemouth (1914), each of which formed an attractive centre and possessed plenty of clinical material. Northampton, at which a meeting was held in 1913, seems to have been selected owing to the clinical enthusiasm of the provincial secretary, Mr. N. B. Odgers.

Officers of the Section.

With the exception of Dr. George Carpenter, who held office during the first two years (1908-9 and 1909-10), the appointment of President of this Section has been for one year only, and surgeons as a rule have alternated with physicians. Of my seventeen predecessors ten have been physicians and seven surgeons, the regular alternation having been always observed except when Dr. Carpenter, as already stated, was president for two years in succession and was succeeded by two physicians, Dr. E. Cautley and Dr. G. A. Sutherland, and again in the session of 1918-19, when Dr. H. Morley Fletcher was succeeded by Dr. J. Porter Parkinson. It is pleasing to note that during the eighteen years of the Section's existence only four presidents have died, viz., two physicians, Dr. George Carpenter and Dr. Leonard Guthrie, and two surgeons, Mr. T. H. Kellock and Mr. Sydney Stephenson.

The delivery of presidential addresses before this Section is an innovation introduced five years ago by Sir Robert Jones, who chose for his subject the treatment of paralysis in children (April 22, 1922). The next address was by Dr. Eric Pritchard on the pathogenesis of rickets in the following October, and the third and last on May 23, 1924, by Mr. R. H. Anglin Whitelocke on posture in the treatment of disease. It has always been a tradition of this Section, as it was of the old Society, that one of the secretaries should be a physician and the other a surgeon, and this rule has been observed with one exception throughout the eighteen years of the Section's existence, even during the war, when surgeons were at a premium and very few surgical cases were exhibited. It was only during the two years immediately following the war that both the secretaries were physicians. The success of a Society or Section depends so much on the energy and enthusiasm of its secretaries that it is gratifying to find that twelve of the eighteen presidents had formerly been

secretaries of the Section or of the old Society, or to put it differently, of the twenty-seven secretaries since 1908, seven have been elected president.

Not the least in importance of the officers of the Section is the Editorial Representative, for on him mainly devolves the duty of keeping up the high standard of the *Proceedings* of the Section. On principle it is desirable that the same individual who shows a taste and ability for such work should continue to hold this self-sacrificing office for as many years in succession as possible. This desideratum has indeed to some extent been realized, as is shown by the fact that there have been only ten such representatives since 1908. No one, however, has been found to equal in energy and devotion the late Dr. George Carpenter, who was honorary editor of all the eight volumes of the *Reports of the Society for the Study of Disease in Children.*

Lastly, the representative on the Library Committee holds an important post, as it rests with him to press the claims of the Section to secure pædiatric works which may involve a considerable outlay of the Society's funds. In this case also it is desirable that the post should be held by the same member as many years in succession as possible, especially if he be one who, like our present representative, Dr. Vincent Dickinson, now in his ninth year of office, has a good knowledge of foreign literature. It is noteworthy that there have been only three other library representatives in the history of the Section, all of whom have subsequently been elected president.

Work of the Section.

The work of the Section may be considered in order of importance under the three headings of special discussions, some of which in recent years have been held in conjunction with other sections, the exhibition of cases and specimens, and short papers.

In the choice of subjects for special discussions, which form one of the most attractive features of the Section, the Council has always shown a wise discrimination and has been fortunate in securing specialists to open the debate. In the domain of infectious diseases discussions have been held on whooping-cough (1908), the portals of entry in phthisis (1911), "botulism" (1918), for which the first cases of encephalitis lethargica were mistaken, and the late results of encephalitis lethargica (1923).

The engrossing topic of infant feeding has twice formed the subject for discussion. The first discussion was on February 20, 1920, when Dr. E. Mellanby opened a debate on "The Importance of Accessory Food Factors—Vitamins—in the Feeding of Infants," and was followed by Professor Noël Paton, Dr. R. Hutchison, Professor C. J. Martin, Dr. Eric Pritchard and others. The second occasion was on April 24, 1925, when Dr. Leonard Findlay, followed by Drs. Eric Pritchard, H. Thursfield, R. C. Jewesbury, and L. Parsons, discussed the question whether modification of cow's milk was necessary in infant feeding.

On December 13, 1912, a discussion was held on the modern methods of treatment of heart disease. Dr. E. Cautley, who in his opening address emphasized the importance of rest, was followed by Dr. (afterwards Sir) James Mackenzie, who dealt with "some manifestations of a healthy heart frequently mistaken as indications for treatment, with a discussion of the significance of various forms of irregularity and murmurs." Subsequent speakers included Sir James Goodhart and Drs. John Hay, Alexander Morison, G. A. Sutherland and F. W. Price.

In the period immediately following the war one of the best attended discussions was on "The ætiology, prevention and non-operative treatment of adenoids," which

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occupied two meetings (January 24 and February 28, 1919). Dr. Harry Campbell, who opened the discussion, emphasized the importance of intestinal indigestion as the principal cause of adenoids, and insisted on the importance of a proper diet, especially one containing more raw vegetable food in the form of salads and fruit. He was followed by seventeen other speakers, including Members of the Sections of Laryngology, Odontology and Otology, though at that time the arrangement of combined meetings with other Sections had not yet been formally introduced.

The most important discussion, however, to which some attention has since been given in foreign literature,[1] was that held on February 25 and March 18, 1921, on the diagnosis and treatment of congenital syphilis. Sir Humphry Rolleston, who opened the discussion, dealt with the limits of the Wassermann reaction as a means of diagnosis, and the influence of congenital syphilis (1) in favouring the onset of other infections, (2) in leading to changes in the endocrine glands and so indirectly to syndromes not necessarily specific, and (3) in relation to diseases or syndromes which are generally regarded as due to syphilis and are not obviously secondary to lesions of endocrine glands. Mr. O. L. Addison dealt with the surgical manifestations of congenital syphilis. Sir Frederick Mott showed lantern slides of juvenile general paralysis and of the testes in the infantile and non-infantile type of the disease, and Dr. L. Findlay, Dr. D. Nabarro and Mr. J. Adams discussed ante-natal and post-natal treatment.

The obstetric and ante-natal aspects of congenital syphilis were considered by Dr. Amand Routh, and Dr. R. C. Jewesbury related his experience at the special clinic at St. Thomas's Hospital for children suffering from venereal disease; the effects of syphilis on the eye with associated symptoms, including nerve reflexes, were related by Mr. N. Bishop Harman, the results of treatment of interstitial keratitis were reported by Mr. H. Neame and Mr. G. R. Woodhead, the teeth in congenital syphilis formed the subject of a communication by Mr. A. T. Pitts, and Dr. Ronald Carter dealt with congenital syphilis from the general practitioner's standpoint.

On more than one occasion subjects appealing alike to the physician and surgeon have been chosen for a discussion, such as intussusception in children (Portsmouth Meeting, 1910), in which Messrs. Childe, Douglas Drew, Fitzwilliams, Lockhart-Mummery and Lett represented the surgeons, and Drs. Cautley, Spriggs and Pernet the physicians. Another medico-chirurgical subject was enlargement of the spleen in children (December 12, 1913), in the discussion on which Dr. Robert Hutchison was followed by Sir John Bland-Sutton and Mr. Philip Turner, who dealt with the surgical aspects, while Drs. G. A. Sutherland, F. J. Poynton, H. Thursfield and F. Parkes Weber continued the discussion of the medical interest of splenomegaly.

The subject of splenic enlargement in children was again chosen for discussion on November 27, 1925, when physicians only took part in the debate, the speakers being Sir Humphry Rolleston (the opener), and Drs. Sutherland, Parsons, Poynton, and Parkes Weber. On only one occasion (December 15, 1911) was an exclusively surgical subject chosen, viz., the treatment of tuberculous joint disease in children, when Mr. A. H. Tubby, who dealt with the indications for surgical interference and the nature of the operations required, was followed by Sir Anthony Bowlby, who discussed the results obtainable without operation, Mr. (afterwards Sir Robert) Jones, who emphasized the importance of immobilization, and Mr. (afterwards Sir Henry) Gauvain, who also supported conservative treatment. Other subjects for discussion

[1] Jadassohn's "Handbuch der Haut- und Geschlechtskrankheiten," Bd. xix, "Kongenitale Syphilis," 1927.

have been treatment of the rheumatic child (Reading meeting, June, 1921), status lymphaticus (Bournemouth meeting, June 6, 1914), and enuresis (March 28, 1924).

An innovation, introduced three years ago, which has met with a considerable degree of success, has been the arrangement of joint meetings with other Sections of the Society to discuss a subject of common interest. The first of this kind was a meeting held on March 6, 1924, in conjunction with the Sections of Neurology, Obstetrics and Gynæcology, and Orthopædics, on birth injuries, with special reference to intracranial injuries with hæmorrhages, and to nerve injuries. The debate was opened by Dr. Cuthbert Lockyer, President of the Section of Obstetrics, who alluded to the classical papers of W. J. Little and Herbert Spencer, and was followed by Dr. Eardley Holland, who gave an account of the ætiology and morbid anatomy of intracranial birth injuries and hæmorrhage. Dr. H. C. Cameron, speaking for this Section, discussed the question from the clinical standpoint under the three headings of (1) the symptomatology of subdural hæmorrhage in the new-born, (2) the survival of certain infants who have suffered in this way, and (3) the ultimate effect upon the physique and intelligence of the child. Subsequent speakers, including Dr. James Collier, representing the Section of Neurology, Mr. H. A. T. Fairbank, of the Section of Orthopædics, who dealt with birth palsy, and Dr. Thomas Lumsden and Dr. Bernard Myers, who described the effects of hæmorrhages into the brain stem, with the report of a case.

The best attended of all those joint discussions in which our Section has taken part was that held on January 13 and 27, 1925, in conjunction with the Sections of Psychiatry, Neurology and Epidemiology, on the mental sequelæ of encephalitis lethargica, when there was an audience of eighty-four on the first occasion and of thirty-one on the second. The debate was opened by Professor Edwin Bramwell, of Edinburgh, who dwelt on the differences between the disease in the adult and in the child, and was followed by Professor Arthur J. Hall, of Sheffield, who classified the mental symptoms of the more chronic phases under the four headings of: (1) the idiot group, (2) the Parkinsonian group, (3) the apache group, and (4) the difficult child group. Interesting statistics were brought forward by Dr. F. C. Shrubsall from the London School Medical Service illustrating the sequelæ among 119 children who had had encephalitis lethargica in 1924. The importance of a guarded prognosis was stressed by Dr. J. C. Spence, and the question of treatment dealt with by Drs. A. S. McNalty, Allan Parsons, and P. C. Cloake.

Pneumococcal peritonitis was the subject chosen for a joint discussion with the Section of Surgery on January 23, 1925, when Dr. H. C. Cameron described the clinical picture and indications for operation, and was followed by Mr. G. Waugh, who considered the surgical aspects, and Dr. David Nabarro, who gave an analysis of cases treated at the Hospital for Sick Children, Great Ormond Street, from 1914 to 1924 inclusive.

The next joint meeting was held with the Sections of Comparative Medicine and Epidemiology on February 9 and March 2, 1925, the subject being the control of tuberculosis and the milk supply. At the close of the meeting a resolution was carried unanimously that "the Council of the Society should be urged to press the Government to re-enact the Tuberculosis Order as soon as possible with a view to reducing the incidence of and mortality from tuberculosis in children and the provision of a pure milk supply."

The last joint debate in the period under consideration was in conjunction with the Section of Dermatology on the ætiology and treatment of infantile eczema, on December 17, 1925. The discussion was opened by Dr. A. M. H. Gray, of the Section of Dermatology, and continued by Dr. H. C. Cameron, followed by Drs. H. G. Adamson, J. H. Sequeira, S. E. Dore, and J. M. H. Macleod, the only other

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representative of this Section taking part in the discussion besides Dr. Cameron being Dr. F. Langmead. A lively debate ensued, in which the relative importance of external irritation, diathesis, individual reaction, and death in eczema were considered.

Clinical meetings devoted to the exhibition of a set of cases illustrating some particular condition form a transition between special discussions on a definite subject and the ordinary meetings at which a more or less great variety of cases are shown. Hitherto only two such meetings have been held, and both in the course of a few months. On the first occasion (January 26, 1923) children presenting the late results of encephalitis lethargica were shown, while the second meeting, held on May 25, 1923, was devoted to examples of birth injuries. It is highly desirable that we should have more such meetings, the success of which will mainly depend on the energy of the secretaries.

Coming now to the consideration of individual cases shown at the ordinary meetings of the Section, we find that while practically every department of clinical pædiatrics is represented, certain subjects have received much more prominence than others. An analysis of the cases brought before the Section since 1908 shows that the following conditions were most frequently the subject of demonstration: Diseases of the nervous system (142 cases), congenital defects, exclusive of congenital heart disease (127 cases), alimentary disorders (63 cases), diseases of the skin (51 cases), congenital heart disease (45 cases), ophthalmology (35 cases), acute infections (33 cases).

The high proportion of cases illustrating nervous and cutaneous diseases is remarkable in view of the fact that the Sections of Neurology and Dermatology have formed part of the Royal Society of Medicine since its foundation in 1907, and is to be accounted for by the fact that while some of these cases were brought forward by professional neurologists or dermatologists, others were shown by pædiatrists who had not specialized in diseases of the skin or nervous system.

Although nine new Sections, viz., those of Balneology and Climatology, Comparative Medicine, History of Medicine, Ophthalmology, Orthopædics, Psychiatry, Tropical Diseases, Urology, and War have been added since 1908, with the exception of ophthalmological and orthopædic cases, which are now rarely brought to our meetings, the Section has not suffered much loss of clinical material from the formation of these new Sections.

As regards diseases of isolated viscera, the liver (30 cases), spleen (12 cases), kidney (9 cases) and lung (5 cases), have received attention in the order named. Apart from congenital diseases, only nine children have been shown illustrating disorders of the heart, but it must be remembered that a special discussion was held on the treatment of heart disease in children, while short papers have been read by Dr. G. A. Sutherland, on acute rheumatic carditis (April 24, 1914), and by Dr. C. P. Lapage and Dr. W. J. S. Bythell, on tonic and atonic hearts (February 24, 1922).

It is a matter of regret that since the war there has been such a decline in the number of surgical cases shown. Whereas in the pre-war period 1908-1913, there were sixty-six such cases brought before the Section, from 1914-1926 there have been only seventeen, in several years there being none at all.

I have previously[1] deplored the comparative rarity with which reports of instructive cases of acute infectious diseases figure in our *Proceedings* in contrast with these of other countries where they take a prominent place. This neglect of the acute infections is partly due to the fact that in London most children suffering from these diseases are sent away as soon as possible to fever hospitals

[1] *Proceedings*, 1925, xx (Child. Sect.), 35.

and thus escape the observation of professional pædiatrists. But this can to some extent be remedied by encouraging the medical staff of fever hospitals to join the Royal Society of Medicine, or at least this Section, where a record of interesting cases of zymotic diseases would be welcomed.

Before leaving the subject of clinical cases it may be noted that the attention of pædiatrists in this country was first drawn to the following conditions by their exhibition at this Section:—Renal infantilism (H. Morley Fletcher, *Proceedings*, 1910-11, iv, Children's Section, p. 95; R. Miller, *ibid.*, 1911-12, v, Children's Section, p. 38); blue sclerotics and brittle bones (J. D. Rolleston, *ibid.*, 1910-11, iv, Children's Section, p. 96); treatment of congenital syphilis by "606" (Sydney Stephenson, *ibid.*, 1912-13, vi, p. 111; J. Bunch, *ibid.*, p. 173); Calvé's pseudo-coxalgia (R. C. Elmslie, *ibid.*, 1912-13, vi, Children's Section, p. 102); mitral dwarfism (F. P. Weber, *ibid.*, p. 129); lipodystrophia progressiva (F. P. Weber, *ibid.*, 1915-16, ix, Children's Section, p. 117; 1916-17, x, Children's Section, p. 81); Köhler's disease (P. Maynard Heath, *ibid.*, 1919-20, xiii, Children's Section, p. 17); œsophagectasis (F. Langmead, *ibid.*, p. 43), and the pink disease (H. Thursfield and D. Paterson, *ibid.*, 1921-22, xv, Children's Section, p. 27; F. P. Weber, *ibid.*, p. 29).

As regards the exhibition of pathological specimens I have always thought it regrettable that so few have been shown at the ordinary meetings of the Section. This has been particularly noticeable since the war, as is proved by the fact that during the six years preceding it (1908-1914) forty-five specimens were exhibited as compared with only thirty-five in the subsequent twelve years. As might be expected, examples of congenital defect were most frequently exhibited (14), neurological specimens came next (13), and then in order of frequency specimens of alimentary disorders (10), acute infectious diseases, congenital heart disease, and diseases of the liver (7 each), kidneys, spleen and suprarenals (5 each), acquired heart disease (4), and eye diseases (1).

Lastly, a few words may be said about the short papers read before the Section. These have embraced a great variety of subjects, including not only infant feeding, alimentary diseases, morbus cordis, congenital defects, ophthalmological conditions, tuberculosis, syphilis and rickets, but also school hygiene and the history of medicine. Among the more important papers may be mentioned those of Dr. F. Langmead on the use of citrated milk in wasting infants (*Proceedings*, 1909-10, iii, Child. Sect., p. 113), Dr. F. Parkes Weber on splenomegaly with recurrent jaundice, ending in hepatic cirrhosis, with remarks on the splenomegaly of inherited syphilis in children (*ibid.*, 1910-11, iv, Child. Sect., p. 62), Dr. David Forsyth on the history of infant feeding since Elizabethan times (*ibid.*, 1910-11, iv, Child. Sect., p. 110), Dr. Leonard Guthrie on epidemic catarrhal jaundice (*ibid.*, 1912-13, vi, Child. Sect., p. 48), Major R. McCarrison on nervous cretinism (*ibid.*, 1913-14, vii, Child. Sect., p. 48), Dr. J. Lawson Dick on the teeth in rickets (*ibid.*, 1915-16, ix, Child. Sect., p. 83), and Dr. E. A. Cockayne on congenital defect of the duodenum (*ibid.*, 1916-17, x, Child. Sect., p. 127).

In conclusion, I would venture to urge that, while we have reason to congratulate ourselves on our contributions to pædiatrics we should not allow our activities to be dulled by self-complacency. The chief improvements which I would suggest are: (1) To have at least one meeting a session devoted to a special subject illustrated by clinical cases, (2) to urge surgical members to exhibit more cases, (3) to encourage the exhibition of cases or specimens or communication of short papers relating to acute infectious diseases, and (4) to have more pathological specimens brought before us.

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[*May* 27, 1927.

Acholuric Family Jaundice.

By K. H. TALLERMAN, M.C., M.D.

I. S., FEMALE, aged 9 weeks, was brought to hospital on account of constipation and jaundice. The jaundice was said to have been present at birth, but to have disappeared. It re-appeared at 6 weeks of age, and since then has remained constant.

Full-term normal child.

Family History.—Mother's only child: one healthy stepchild in family. The mother suffered from jaundice all her life, but underwent splenectomy in 1925. Since then she has been completely free from jaundice. The child's maternal grandfather is said to have always suffered from bilious attacks. He, himself, first noticed jaundice in 1918, but his daughter says that he has always been jaundiced, and he is definitely so at the present time. The maternal great-grandfather is also reputed to have suffered from jaundice and bilious attacks.

On Examination (March 15, 1927).—The child appeared extremely anæmic, and there was a definite icteric tinge of the skin and sclerotics. Liver not enlarged. Spleen two to three fingers' breadth below costal margin.

Blood-count: red cells, 3,100,000; hæmoglobin, 40 per cent.; colour index, 0·64; white cells, 9,720; polymorphs 23 per cent., small lymphocytes 53 per cent., large lymphocytes 13·5 per cent., large hyaline cells 5·5 per cent., eosinophils 5 per cent. Eleven normoblasts and two megaloblasts seen during the count of 200 white cells. Poikilocytosis, anisocytosis and polychromatophilia. Fragility of red cells increased. Hæmolysis stops at 0·65 per cent. saline with patient's cells and at 0·45 per cent. in control.

Urine normal; shows only a small amount of urobilin, not sufficient to be called pathological. Stools normal in colour.

Re-examined, May 11, 1927.—Colour slightly more normal. Physical examination essentially the same, but liver just palpable.

Another blood-count on this date showed but slight variation from the previous count; the leucocytes, however, were at this time 12,960, with 38 per cent. polymorphonuclear cells and 39 per cent. small lymphocytes.

Wassermann reaction negative.

The child is steadily gaining in weight, seems to be improving in general health, and is no longer constipated.

The case is of interest, in view of the age of the patient, and the fact that a history of the condition in the family appears to extend over four generations.

Myeloma of Humerus.

By E. A. CROOK, F.R.C.S.

G. B., MALE, aged 13 years. This child fell on his right shoulder in August, 1925. Signs of an impacted fracture of surgical neck of right humerus were found, and confirmed by X-ray examination. Sound union and full restoration of function followed. On May 13, 1927, he fell on his right shoulder in the street again. There were signs of fracture in upper third of right humerus, and expansion of bone was felt. The X-ray shows a spontaneous fracture through a myeloma.

This case should be interesting on account of the original injury which the child sustained two years ago, when the X-rays showed no evidence of myeloma. Fracture of the surgical neck healed up readily, and we did not see the child for

surgical examination until the 13th of May, when he came after a fall in the street, which was borne by the right shoulder. Apparently it was a spontaneous fracture. There were signs of this bony expansion, and the X-rays show either a myeloma or fibro-cystic disease of the bone. The point of interest is the question of relationship

between the original injury and the present condition; is there such relationship, or is it a pure coincidence? There is a history of rheumatism; the child suffered from chorea and rheumatism for four years. At one time there was an acute rheumatic condition in the right hip-joint, but it cleared up, and the X-rays do not at present reveal any abnormality.

64 Crook: *Diaphysial Aclasia*

Diaphysial Aclasia.

By E. A. Crook, F.R.C.S.

J. J., MALE, aged 9 years. This child was admitted to hospital in February, 1926, with subacute appendicitis, when he was found to have multiple exostoses at the

ends of his long bones. He also had marked genu valgum. In August, 1926, during massage, one of his exostoses was broken off. The fracture united in the usual time.

The appendicitis was satisfactorily dealt with. X-ray examination showed that every long bone in the body had these irregular growths close to the epiphyseal line. The child showed evidence of past rickets, which, apparently, is a factor in the production of these irregular growths. There was considerable laxity of the ligaments of the knee-joints, and he has had treatment for the genu valgum.

Discussion.—Dr. F. PARKES WEBER said he thought that in regard to cases like the present one it was convenient to use the old term "multiple exostoses," which neither implied true tumour-formation nor depended on any theory of origin, as did Sir Arthur Keith's term "diaphyseal aclasis or aclasia." Aplasia or hypoplasia of the distal end of the ulna was often present in these cases, and it was not easy to understand how this absence could account for any defect in "pruning" or "modelling" on the part of the periosteum (such as was implied by Sir Arthur Keith); one could not model anything that was absent. He (Dr. Weber) had suggested another term.[1]

In most cases of multiple exostoses, as in the present one, the distal extremity of the ulna partially or completely failed to develop, but there were likewise (two) cases of multiple exostoses in which this characteristic defect of the ulna was not present, and also (three) rare cases in which the defect of the ulna could be demonstrated by Roentgen skiagrams, though no definite exostoses could be detected.[2]

Dr. J. KINGSTON BARTON said this was a question which was continually being discussed, and it was a matter on which the views of embryologists should be ascertained. In this case there appeared to be an absence of the periosteal limit to the modelling material composing the bones, this absence allowing of the bone substance pushing itself out in various parts, running riot as it were. These exostoses were more marked at the distal extremity of the forearm or the upper extremity of the upper arm, whereas in the legs the reverse was the case. It must be regarded as a developmental disease.

Case for Diagnosis.

By STANLEY WYARD, M.D. (shown by DOUGLAS FIRTH, M.D.).

I. H., AGED 12 years.

History, 1925.—Pains in limbs.

March, 1927.—Easily tired with occasional pains.

Past History.—Nothing of importance.

Condition on Examination.—Heart slightly enlarged.

April 1, 1927.—Loud diastolic murmur heard all over chest, front and back. Diastolic thrill. Enlarged tonsils.

April 12, 1927.—Tonsillectomy.

Last fourteen days.—No pyrexia. Pulse, regular, 72-100. Steady increase in weight. Heart: as before.

Dr. FIRTH said that both diagnosis and prognosis were difficult. During the last Session he brought before the Clinical Section[3] a child showing physical signs which closely resembled those found in this case of Dr. Wyard. The child was then in hospital under his care, and he had considered the possibility of a congenital lesion, mainly because the physical signs did not agree with any acquired lesion. But when, on seeking the opinions of his colleagues, one confidently said the condition was mitral stenosis, a second was equally certain the sound was exocardial, and a third gave it up entirely, he sought further opinions at the Clinical Section. There most Members fought shy of expressing anything definite, but Dr. Vincent Coates said he considered the child was rheumatic, and he was inclined to

1 "Periosteo-osteo-dysplasia." See F. Parkes Weber, "Diaphyseal Aclasis or Periosteo-osteo-dysplasia (Multiple Exostoses) with Shortness of Forearms," *Proc. Roy. Soc. Med.*, 1924, xvii (Clin. Sect.), 40.

2 F. Parkes Weber, "A Developmental Deformity of the Right Forearm, similar to that sometimes met with in Multiple Exostoses," *ibid.*, 1925, xviii, 25.

3 *Proceedings*, 1926, xix (Clin. Sect.), 26.

the idea that the murmur was exocardial. Owing to various circumstances, he had failed to keep in touch with the patient, but he saw her again last week, and brought her before the Section for comparison with Dr. Wyard's patient.

She had recently complained of more aches and pains, had been off colour and languid, and was not so well as she was last year. The physical signs had only changed to this extent, that the musical pitch had disappeared from the murmur, which even last year was very variable.

Some years ago he had seen another case like these, a girl of about the same age; she was under the care of Dr. E. I. Spriggs, who labelled the murmur "taxi-cab," and who, to the best of his recollection, thought the condition was mitral stenosis. All trace of this child had been lost.

His own view was that these children were rheumatic, and so was the cardiac condition, but what the latter was he would not like to say. He did not think there was active endocarditis, but a period of observation was necessary before reaching any real conclusion as to the origin of the murmur and the prognosis. In his case the electrocardiogram and the skiagram were normal.

Case of Stunted Growth.

By D. W. WINNICOTT.

E. A., MALE, aged 5 years.

Family History.—Two other children are well and two have died, one from "meningitis" and one from "? mastoid." There was one miscarriage.

History.—He has always been backward, both physically and mentally. He began to talk at three years of age, but made slow progress. He can now speak simple sentences. His only illness has consisted of frequent attacks of bronchitis. He has not yet control over micturition.

Examination.—Weight 21 lb. He is small and thin. He always looks anxious. When seen for the first time on April 21 he had had quite definite asthma. After admission to the wards he had no more asthma but there was some bronchitis with fever. The chest is altered in shape to a degree suggesting that in the previous attacks of bronchitis there was an asthmatic element. There is some degree of clubbing of fingers and there are very marked epicanthic folds.

There are no symptoms or signs pointing to renal or intestinal infantilism, and the facies does not remind one of any well-described group.

Wassermann: Negative in blood and cerebro-spinal fluid.

Routine blood, cerebro-spinal fluid, and urine examinations: all normal (Dr. E. O'Flynn).

I hope that some Member will be able to help me to decide what are the causes of stunted growth in this case. The premature birth accounts for a part of the smallness and general backwardness; the presence of nasal obstruction from birth, and the occurrence of repeated attacks of bronchitis since the sixth month with (at any rate in the last attack) an asthmatic element, account for a great deal of the rest. But the epicanthic folds and the abnormal condition of the little fingers and little toes perhaps point to a further factor, namely, a congenital defect—although there is certainly no suspicion of mongolism. This further factor would greatly affect prognosis, which, in turn, affects treatment in the case of a backward child.

Dr. G. A. SUTHERLAND said he agreed that this child had not been normally progressive since birth. The congenital defects were evident and interesting. Though the marked epicanthic folds and the shortening of fingers and toes were definitely associated with mongolism, he was sure that those two defects were sometimes seen in children who apparently were otherwise normal. He would not lay too much stress on those defects, as he thought much of the bad health could be attributed to the early bronchitis, which would inevitably retard development if it were a chronic condition.

Case for Diagnosis.

By F. J. POYNTON, M.D. (shown by BERNARD SCHLESINGER, M.B.).

W. L., MALE, aged 1 year 1 month.

History.—Admitted to University College Hospital on May 13, 1927, with convulsions. On the two preceding days the child had been suffering from a cough and drowsiness.

Family History.—Father subject to fits until seven years old.

Condition on Admission.—The convulsions were generalized and were overcome by inhalation of chloroform. Following the convulsions a flaccid left hemiplegia continued for twelve hours. On the next day the central nervous system had completely recovered, and the temperature which had been 105° F. on admission had subsided to 99° F. to 100° F. The child remained irritable for a few days, the throat was injected and the cervical and occipital glands somewhat enlarged.

Respiratory, cardiovascular and alimentary systems all normal.

Course of the Disease.—There were further convulsions five days later on May 18. The first fit was generalized, but the two subsequent fits were confined to the left side, and during these last two he did not lose consciousness. Treated with bromide per rectum.

Four days later the left arm was suddenly noticed to become stiff, this became normal again and remained so for six hours, and since then has taken up the present position.

Present State.—The left arm is held almost continuously pronated with elbow extended and shoulder extended backwards, fingers extended and abducted. The arm feels spastic but occasionally relaxes by itself. The patient does not move the arm at all and when abducted from the side it falls back into the original position. Athetoid movements are occasionally noticed in the fingers of the left hand. No wasting of the muscles of the arm. Sensation appears normal. No pain in left arm. Legs show normal motor power, and the child can stand, although rather clumsily on the left leg. There are signs of rickets present.

Reflexes

	Right	Left
Ankle-jerk	nil	nil
Knee-jerks	+	+
Plantars	Flexor	Flexor
Biceps	+	+
Triceps	+	+
Supinator	Not obtained	Not obtained

Abdominals all present
Discs normal

Dr. BERNARD SCHLESINGER: The diagnosis suggested is one of a central lesion, probably polio-encephalitis, which I presume has affected the basal ganglia, causing some athetosis and spasticity in this limb. I cannot explain why this child has taken up the posture usually associated with Erb's paralysis affecting the fifth and sixth cervical roots.

I shall be glad of opinions as to the nature of the case, and the prognosis. At present the child is being treated with a splint, the limb being put in the posture of maximum rest.

Subacute Infective Endocarditis in Congenital Morbus Cordis.

By Bernard Schlesinger, M.B.

Boy, aged 14 years.

History.—Weakness, shortness of breath and pains in knees for one month; influenza one month ago. Felt quite well until influenza attack, although known to have congenital heart disease.

Physical Signs.—*Heart:* L.B.C.D. enlarged ½ in. to left of nipple line. Cardiac impulse 5th space. R.B.C.D., 1 in. to right of sternum. Thrill palpable over pulmonary area. *Murmurs:* Systolic at apex. Continuous systolic and diastolic heard at the base of the heart; heard loudest at pulmonary area. Temperature, 99-102° F. Spleen palpable. Fingers clubbed. Petechiæ have been seen.

Blood Count.—Red blood cells, 3,270,000; hæmoglobin, 58 per cent.; colour index, 0·89; white blood cells, 11,400; polymorphs, 75 per cent.; lymphocytes, 24 per cent.

Blood Culture.—Abundant growth of non-hæmolytic streptococci.

Dr. Bernard Schlesinger: I have brought forward this case in order to ask members of the Section whether they find these cases are more common than in the years before the war. In adults infective endocarditis has been more commonly found since the war than before. The fact that it is superimposed upon congenital heart disease is well known. In some cases the latter may only be found post mortem, when, perhaps, there may be found two valves in the aortic area, instead of three, or there may be small congenital disorders, which would have escaped notice if infective endocarditis had not supervened and had been the cause of the case being brought to autopsy.

Section of Dermatology.

President—Dr. J. H. Sequeira.

Ichthyosis and Cancer of the Skin, with Special Reference to Mulespinners' Cancer.

By Louis Savatard (Manchester).

It is generally agreed that xeroderma pigmentosum may be regarded as a pre-cancerous dermatosis, since the supervention of carcinoma, in this rare disease, is the rule rather than the exception. But the fact that xeroderma simplex or ichthyosis simplex is also a fertile soil for malignant growths is not, I think, sufficiently appreciated.

In this brief communication I wish to stress the rôle of ichthyosis as an important ætiological factor in carcinoma of the skin.

For some years I have made it a routine practice in all cases of cancer of the skin to examine the whole surface of the skin and in some few cases I have been rewarded by finding multiple lesions which were unknown to the patient. I have found, too, that in many cases of carcinoma there was an associated general ichthyosis. More especially has this been the case in patients presenting carcinomata which have arisen on a pre-existing keratosis. So frequently have I found this association of keratosis with ichthyosis, that I am led to believe that the keratosis is secondary to the ichthyosis and that their association is not a simple coincidence, and further, that many cases of so-called senile keratosis, of sailor's skin and of biotripsis (Cheatle) would on a general examination of the skin be found to be cases of keratosis in ichthyotics. I do not suggest that these conditions may not arise in the absence of ichthyosis. I am fully aware that they do and will shortly show you examples.

Since cancers frequently develop on keratosis in persons over middle age and since keratosis is a marked feature of ichthyosis we might naturally expect to find a predisposition to carcinoma in this condition; the sequence of events being ichthyosis —keratosis—carcinoma.

All types of carcinoma may develop on keratosis—from the superficial atrophic basal to the more rapidly-growing squamous.

The keratotic lesion may be a nodule of pinhead dimensions or may present the appearance of a plaque, which from time to time sheds its surface epithelium. Many of these keratotic nodules and plaques, though apparently benign, show, on histological examination, evidences of well-established malignancy. Keratosis is frequently the first clinical evidence of a squamous carcinoma (epithelioma) of the skin, whether produced artificially or arising *de novo*, and is occasionally the first indication of a basal carcinoma (rodent ulcer).

I now show lantern slides of cases to illustrate the foregoing remarks:—

Case I.—Female, aged 74, whom I saw six years ago. A squamous carcinoma had developed on a keratotic plaque on the left side of her nose. You will note, too, the keratotic lesions in the left temporal region. I cannot remember that she suffered from ichthyosis. Patient has since died.

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Case II.—Female, aged 76, in hospital at present. The picture shows several keratotic plaques on the forehead and scalp and on four of these rodent ulcers have developed. There is no evidence of an ichthyotic skin (fig. 1).

Case III.—Male, aged 54, whom I saw first six years ago. He then presented a small rodent ulcer in the right malar region. Last year his doctor sent him to me again on account of a keratotic plaque on the right side of his forehead. You will note other keratotic foci on his face and atrophic lesions on the nose and left cheek—the sites of previous keratotic lesions. He suffered from general xeroderma.

Case IV.—Male, aged 74, who presented a superficial rodent ulcer of the nose on a keratotic base (fig. 2). There were numerous other keratotic nodules, white atrophic patches, pigmented areas and telangiectases on the face; so that the picture was very suggestive of an acquired xeroderma pigmentosum. He had well-marked general ichthyosis.

Case V illustrates a squamous carcinoma secondary to a so-called senile wart on the dorsum of the left hand between the index and middle finger. You will note the scaling and pigmentation on the back of the hand. The patient suffered from ichthyosis.

Case VI shows the backs of the hands of a man aged 80—a carpenter by trade—who suffered from ichthyosis. You will note the numerous keratotic nodules and one on the right hand in which a squamous carcinoma had supervened. On the face there were also keratotic nodules, rodent ulcers and an epithelioma.

Case VII.—The next three slides illustrate three different conditions in an elderly female who was the subject of ichthyosis:—

(*a*) Shows numerous keratotic lesions on the dorsum of the left hand.

(*b*) Shows a so-called pre-cancerous dyskeratotic lesion of the right thigh in the centre of which a cherry-red tumour had arisen; this on section showed early malignancy.

(*c*) Shows a rodent ulcer on the right side of the neck.

Case VIII.—Another case of pre-cancerous dyskeratosis. The lesion, which simulated a psoriasis plaque, was of ten years' duration and is situated on the left breast, some distance from the nipple. Patient was a female, aged 48, who suffered from ichthyosis.

Is ichthyosis often associated with this pre-cancerous condition?

Case IX illustrates a case of epitheliomata on lupus scars. X-rays had not been administered. Patient was an ichthyotic.

Is epithelioma more likely to arise in lupus patients who are ichthyotic?

Case X shows an early rodent ulcer in a male, aged 27, who also was the subject of ichthyosis.

Dr. Prosser White, of Wigan, has described a condition of the skin in mule-spinners to which he has given the name of "shagreen" skin. Though we see many mulespinners in Manchester we had never noted this peculiarity. The riddle was, however, apparently solved some little time ago when I showed a patient (Case IV of this series) at one of our local dermatological meetings.

The patient had been a mulespinner for many years and Dr. White pointed out that his arms exhibited the "shagreen" skin peculiar to mulespinners and workers in oil. What the patient presented was simple ichthyosis. It was marked not only on the arms but on the trunk and legs, and he had suffered from this condition long before he began work as a mule-spinner.

What my friend describes as shagreen skin is, I believe, simply ichthyosis. If we examine Dr. Prosser White's illustration we find a good example of the arms and hands of an ichthyotic mulespinner. The hyperkeratosis of the palms, which Dr. White attributes to the carcinogenic agent (oil), is characteristic of many congenital ichthyotics but is not characteristic of mulespinners. This patient had had an epithelioma removed from his scrotum, and the same was done in another similar case recorded by Dr. White.

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FIG. 2.—*Case IV.*

FIG. 1.—*Case II.* Showing rodent ulcers on senile keratosis.

FIG. 3.—*Case XI.* The arrow points to an early epithelioma. The upper lesion is a keratotic plaque.

FIG. 4.—*Case XI.* Keratotic plaque just above the elbow. Clinically benign but histologically malignant (*vide* fig. 6). Note the marked ichthyosis of the skin.

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Case XI.—In September last, the patient (a male), whose photograph I now show you, was sent to me on account of a rodent ulcer at the right side of the bridge of his nose. I noted that there were numerous keratotic foci on the face; on further examination found that his skin was markedly ichthyotic and learned that it had been so since infancy. I found, further, a typical early squamous carcinoma, of two weeks' duration, on the flexor aspect of the right forearm (fig. 3) and numerous keratotic plaques on both upper (fig. 4) and lower extremities; sections of some of these apparently benign plaques showed well established squamous carcinoma (figs. 6 and 7). On the left side of the scrotum was a keratotic plaque (fig. 5) which, however, showed no definite malignant development. The patient was aged 67, and had been a mulespinner for many years.

FIG. 5.—*Case XI.* Showing a hyperkeratotic plaque on left side of scrotum—a pre-cancerous hyperkeratosis. Note also the keratotic wart on left wrist (*vide* fig. 7).

My opinion, however, is that these cancers would have developed in this man no matter what his occupation had been. Yet I understand the position to-day is that if a mulespinner develops a cancer of the skin, whether on the scrotum or elsewhere, and whether it be of basal or squamous origin, that cancer is certified as being due to his occupation.

What is the relative frequency of ichthyosis? It is apparently fairly common in Lancashire, but its incidence may vary in different parts of the country. In an examination of 400 operatives I have found a 5 per cent. incidence.

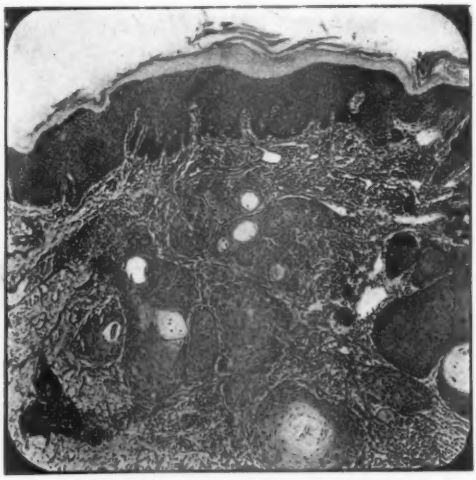

FIG. 6.—Low power magnification of part of section of the keratotic plaque shown in fig. 4, showing well-marked invasion of the dermis by cancer cells.

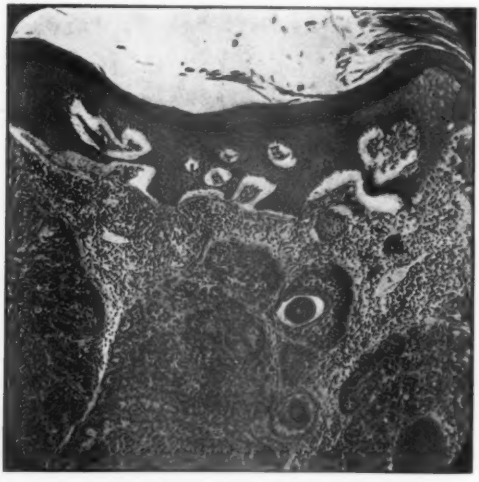

FIG. 7.—Low power magnification of part of section of the wart on left wrist shown in fig. 5, showing well established malignancy.

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Through the kindness of my friend Dr. Robertson, M.O.H. for Darwen, I have recently been afforded the opportunity of inspecting 400 mulespinners at work. Their ages ranged from 15 years to 65 years and the duration of their occupation varied between two days to fifty years. I found twenty affected with ichthyosis. In ten the affection was slight and in the other ten the condition was well marked. Of these, two had suffered from scrotal cancer. Three exhibited keratotic lesions on the face and temporal regions. I saw no such lesions in the non-ichthyotic; hyperkeratosis of the palms was seen only in the ichthyotics. I could find no evidence of any dermatosis of an occupational character, except for two cases of mild folliculitis of the arms in two youths who were also affected with ichthyosis.

As a result of this investigation I believe that ichthyosis is an important ætiological factor in mulespinners' cancer. Though I do not deny that scrotal cancer does arise apart from ichthyosis, I am convinced that the great majority of such patients will give evidence of an ichthyotic skin.

It is not my purpose to discuss the cause of scrotal cancer, but I would express my regret that the Departmental Committee as a result of its inquiry in 1925 should have found that "*in the absence of exposure to mineral oils there would be no mule-spinners' cancer.*" Dr. Robertson, in a very careful inquiry into this subject, comes to quite a different conclusion. I do not deny that mineral oils are carcinogenic agents, but would submit that a thorough examination of the whole surface of the skin should be made in all cases of so-called occupational cancers before arriving at a definite conclusion as to their cause.

The object of this communication will have been attained if I have made out a case for the necessity of a more thorough and systematic examination of the skin in all patients presenting cancerous and pre-cancerous dermatoses.

Discussion.—Dr. J. H. SEQUEIRA (President) thanked Dr. Savatard for his interesting and well-illustrated paper, and for bringing the patient such a long distance to the meeting. Mulespinners' cancer was a rarity to those who practised in London.

Dr. A. WHITFIELD said he thought Dr. Savatard rather over-estimated the importance of ichthyosis. He (the speaker) admitted that the ichthyotic skin was badly nourished; its special characteristics were the absence of true maturity of the horny cell layer and the formation of grease. One of the points in the senility of the skin was that in middle-aged people intracellular grease was not developed to the same degree as in the young. The ichthyotic patient started life with that particular aspect of senility, therefore Dr. Savatard was probably right in thinking it a predisposing cause. In the south of England, dermatologists did not see cases of mulespinners' cancer, but a good deal of ichthyosis was seen, and these latter cases were liable to dermatoses. He had not seen malignant keratosis in the ichthyotic. Generally, as soon as an ichthyotic walked into the room and shook hands with one, one could recognize the condition of his skin, quite apart from his mask-like face. If ichthyosis were a predisposing cause of cancer (e.g., trauma and working in mineral oils), more subjects of malignant ichthyosis would be seen in the south.

With regard to the patient suffering from scrotal malignant disease shown that day, he was clear about this being an early cancerous stage; the reason as to why it differed from the example of frank keratotic carcinoma which Dr. Savatard had shown was, that in the one case the scrotum was affected, in the other the extensor surface of the arm. He (Dr. Whitfield) said it was carcinomatous not because there was a downgrowth, as that might occur in an inflammatory condition, but because there was an advanced metaplasia of the cells; there was a peninsula of cells diving into the corium which had lost their character as epithelial cells. This Dr. Savatard had eventually conceded in conversation. For a cell to have changed so much was strong evidence of malignancy. On tracing the basal layer one found groups of cells which had altered in staining qualities and shape—cells which had not yet grown down, but were about to do so.

Dr. H. MACCORMAC said it had been his experience to observe the basal type of carcinoma originating on apparently normal skin; whereas, in contrast, the prickle-cell type tended to

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develop on some pre-existing condition. He did not mean by this that there was any essential difference between the two types of malignant growth, each merely representing the extreme ends of a scale. He reminded the Section of the relatively high incidence of malignant disease of the skin in Australia, which seemed to be attributable to exposure and light. The term "senile keratosis" was sometimes loosely applied to the true condition developing on the exposed regions of elderly people, and also improperly (in his view) to a "seborrhœic wart." The former was one of the true pre-cancerous dermatoses, whereas the latter very rarely underwent malignant degeneration.

Dr. A. M. H. GRAY said the question which Dr. Savatard had brought forward in his paper was a very interesting one ; it demanded the exercise of much thought, particularly in regard to malignant growths on the face, etc. In the case of scrotal cancer, he (Dr. Gray) thought the question was a simpler one. He understood Dr. Savatard to suggest that oils were not a determining factor in causing cancer of the scrotum, but it should be an easy matter to determine how many cases of carcinoma of the scrotum occurred in people who were not associated with work concerned with oil, tar, pitch or soot. He would have thought it rare to find carcinoma unassociated with those occupations ; he thought they must have some definite bearing on the ætiology of the condition.

Dr. SYDNEY THOMSON said that although he had seen a very large number of ichthyotics of all ages, he did not remember ever having seen a single warty growth of any sort amongst them. Whilst it would be impossible to decide the merits of the question without fuller details about the ages of the ichthyotics in the mills and the incidence of cancerous growths among them as contrasted with the corresponding figures for the normal workers, it must be remembered that ichthyosis had a well-marked topographical distribution, whilst such was not true of the "senile skins." Yet malignant changes in senile keratosis which occurred on the more atrophic skins always took place in people who were exposed to trauma. Therefore, if ichthyosis were an important predisposing cause of such changes, one ought to find them in many ichthyotics who were constantly exposed to sunlight, etc., but this was apparently not the case.

Dr. H. W. BARBER said he had been more interested in ichthyosis in its relation to asthma, prurigo and hay fever; but one point worthy of discussion was the association of multiple superficial basal-celled carcinomata with psoriasis, as pointed out by Dr. Gray. The last three cases he had seen of multiple superficial carcinomata had been in patients affected with psoriasis. Two of them were definitely ichthyotic, and one had a dry skin which did not merit the term ichthyosis. He asked whether Members considered that psoriasis was more likely to occur on a dry skin than in people with a normal skin. He thought that, apart from the not uncommon association with frank ichthyosis, one saw psoriasis more commonly in dry-skinned people with marked keratosis pilaris. Professor Bosellini, in his paper on Bowen's disease, suggested that the apparent association of superficial basal-celled carcinomata with psoriasis was due to a mistaken diagnosis ; he suggested that the patches of so-called psoriasis were really superficial basal-celled carcinomata, an opinion with which he (the speaker) did not agree.

Postscript.—On the day following my making the above communication, Dr. Prosser White presented, at a meeting of the Manchester Dermatological Society, a case of oil dermatosis (shagreen skin) in a non-ichthyotic mulespinner. I wish, therefore, to apologize to Dr. White for concluding that this condition did not occur in mulespinners.

On the same day Dr. Barber wrote me: "This morning I was consulted by an Australian lady with marked ichthyosis and keratoses on the face and back of the hand, which she attributed to the very strong sunlight in Adelaide, where she lives. There are no signs of any actual epithelioma as yet, but I thought the association of ichthyosis, Australia and keratoses might interest you."

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Dermato-phytosis due to Monilia.

By J. M. H. MacLeod, M.D.

THE patient, a woman aged 46, presents on the second, third and fourth interdigital clefts of the right hand patches of white, sodden skin, extending down for about quarter of an inch from the clefts. These patches have been present for several years; they are similar to the white, macerated, thickened skin so familiar between the fourth and fifth toes in ringworm of the feet, and suggest either a trichophyton or epidermophyton infection.

In scrapings of the affected skin, examined in liquor potassæ, long, coarse, mycelial filaments, which did not branch, were detected. On Sabouraud's maltose agar, the organism grew at a temperature of 25° C. as a creamy, glistening culture, which tended later to become yellowish. Certain sugar tests were done, and it was found that there was no change in lactose and maltose, but that in glucose, after a week, the medium became acid and gas was produced.

A microscopic examination of the culture showed round or oval bodies, 6 to 7 microns in diameter, with numerous budding forms, but no asci. At first, mycelium was absent; it could be detected in older cultures, but was never profuse.

The organism has been recognized as being *Monilia pinoyi*. It differs from the *Oïdium albicans*, or *Monilia albicans*, as it is now sometimes called, in that though the cultures are similar, the mycelium in *albicans* is much more profuse and slender, and there is much less gas formation in glucose.

The case is of interest from the fact that, while it has all the appearance of a trichophytic infection, it is due to *Monilia*, and also, owing to the present confusion regarding the subject, it is important that such cases should be carefully studied ætiologically, and the fungus responsible recognized if possible. This confusion has lately been increased by the tendency of certain writers in America to employ the term "epidermophytosis," as including not only infections by epidermophytons, but also infections from trichophytons, monilias, yeasts, etc.

At the present moment the classification of the yeast-like fungi is still undecided, but there seems to be one definite group which can be differentiated owing to the absence of asco-spores and the presence of mycelium, and to which the name *Monilia* is given. To this group belong several different forms of *Monilia*, which, though indistinguishable morphologically, can be differentiated by their capacity for producing gas in various sugar media, and in it may be included the *Oïdium albicans* of thrush. These forms of *Monilia* are capable of causing lesions in the skin and nails of a similar type to those produced by the trichophytons and epidermophytons.

The problem of the true yeast infections is also a difficulty at the present time, as certain writers seem to hold the view that though yeasts are apt to grow in moist and intertriginous regions they do little harm there and are only saprophytic. On the other hand, lesions of the skin, of the type described as "erosio interdigitalis blastomycetica" have been produced by the inoculation of yeast cultures, and Shelmire succeeded in producing acute onychomycosis on himself by the same means.

In the March issue of the *British Journal of Dermatology*, Dr. Avit Scott reported a case of oïdiomycosis, which he attributed to a yeast-like fungus. He was kind enough to send me a culture of the organism, which proved both morphologically and culturally to be a *Monilia*.

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Discussion.—Dr. A. WHITFIELD said he had seen three cases of erosio interdigitalis, from which he had cultivated a yeast-like organism, probably *Monilia*. One case (not included in those three) was that of a lady who was sent on account of paronychia. There was no destruction of either the nail plate or the nail bed. The lesion looked very like streptococcic paronychia. It had been persisting for a long time. He made no cultivation from that case, as he had not sole charge of it, but a *Monilia* was cultivated from it by Dr. Joekes. When *Monilia* occurred in the nail area, it caused paronychia rather than a true onychia. One of his difficulties was how to know whether in a given case of eczematous dermatitis, the dermatitis had subsequently become infected with *Monilia*, or whether the *Monilia* was causative. In the last case he had seen it appeared to him that if it were a matter of eczema due to skin susceptibility he could render the condition worse by using strong preparations of his ointment, whereas if the *Monilia* were causal, the ointment would render the patient's condition temporarily worse but eventually better. The patient agreed to take the risk. He (the speaker) used the ointment and the lesion cleared up, therefore he assumed the *Monilia* was causative.

There was a rather dry form of epidermomycosis of the groin which was caused by a yeast-like organism; many years ago he (Dr. Whitfield) had demonstrated cultures of it. But he found he had been anticipated in the matter by Dr. Castellani, who had seen it in tropical patients. They were apparently true yeasts. All the cases in which he had seen the curtain-like condition had been in men, whereas all the cases of *Monilia* infection he had seen between the fingers were in women. Epidermophytosis was much more common in men, in the groin, than in women, and was on the whole distinctly so when it affected the toes and fingers. It was therefore rather curious that in his experience so similar a condition of the digits should be commoner in women.

In reply to Dr. MacLeod's doubts as to the pathogenicity of the yeasts found in the inguinal dermatitis, he (Dr. Whitfield) said that the organism was abundant in stained scales; a pure culture was easily obtained and the disease yielded to fungicidal treatment. Had Dr. MacLeod any further evidence than this to make him believe that the *Monilia* found in his case was causative?

Dr. H. MACCORMAC said he had recently seen two cases, in sisters, of paronychia, due to the *Oïdium albicans*. The objective appearances were not sufficient to indicate the nature of the infection, and he suspected that this form of disease was possibly more common than was thought, because a systematic bacteriological examination and culture was not always made. In a recent paper eleven such cases were described, excluding that recorded by Dr. Avit Scott in the *British Journal of Dermatology*.

Dr. AVIT SCOTT said that he was afraid he could not add anything to what had already been said by Dr. MacLeod, Dr. Whitfield and Dr. MacCormac. He added that his case was also that of a woman and that she now had four nails affected. He would like to hear suggestions as to treatment.

Dr. J. H. SEQUEIRA (President) said that chronic infections of the nails by ringworm were more common in women than in men, and more common in private than in hospital patients.

Dr. W. N. GOLDSMITH said that in one of his cases he found *Monilia* between the toes in a man. The organism was larger than *Oïdium albicans*, and looked like that shown by Dr. MacLeod. It was not a *Saccharomyces*, as it did not ferment glucose in the few days it was watched. Perhaps women's hands were affected more often than men's, as they more often became sodden from washing, etc. In the case of the feet the sexes were not distinguished in that way.

Dr. MACLEOD (in reply) said he did not think there was any more cause for the occurrence of the condition in women than the constant maceration of the hands by washing. One was always being confronted with the idea that these were yeast conditions, but *Monilia* was not *Saccharomyces*.

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? Pemphigus Vegetans.

By S. E. DORE, M.D.

I AM indebted to Dr. Humphry Ward for sending this case, that of a woman aged 59, who five months ago developed massive, protruding, warty growths on both lips somewhat resembling condylomata. There had been no previous formation of bullæ on the lips or in the mouth. The lesions in the right axilla began three months ago in the shape of raised serpiginous moist patches. No drugs had been taken before the lesions began to appear, but subsequently she had had iodide of potassium and arsenic. The Wassermann is negative, and she has no constitutional symptoms. My tentative diagnosis is pemphigus vegetans, but there are several features which do not conform to this view. Pemphigus vegetans is generally fatal in from two to six months, but to show that a much longer duration does not exclude the diagnosis of pemphigus vegetans, I may mention a case now under my care in Marylebone Infirmary, which was originally under Dr. MacCormac. The condition has been present for two years. There are enormous protruding granular masses in both axillæ and on the buttocks and perineum. They have remained practically unaltered during the two years she has been under observation. The lesions in the axilla, as a result of the application of X-rays, have almost entirely disappeared. While under my observation that patient has never had any bullæ, except once when she developed them on the lips a year ago, but these cleared up and no vegetations occurred. There was, however, I believe, a history of bullæ at the onset of the disease. Therefore I do not think the absence of blebs would exclude the diagnosis of pemphigus vegetans in this case. Further observations will be made with the view of excluding syphilis and iodide as possible causes.

Discussion.—Dr. A. DOUGLAS HEATH said that some years ago he showed a lad with vegetations all over his back, and numerous small epidermic abscesses. Most Members considered it was Hallopeau's disease of mild type, and he thought the present case might be one of that kind. For a long time diphtheroid bacilli were found freely in the mouth. The condition cleared up entirely.

Dr. A. M. H. GRAY said that this seemed to resemble the type of condition described as dermatitis vegetans, rather than true pemphigus vegetans. Some confusion existed concerning dermatitis vegetans and true Hallopeau's disease. He did not think the present case belonged to the latter group. A case of Hallopeau's "pyodermite végétante," originally under Dr. Crocker, afterwards came under his (the speaker's) care, and Dr. Pernet published a long description of it in the *Journal of Cutaneous Diseases*. It was very different in type, as the pustular element was very well marked. This was absent in the present case. The cases described under the term "dermatitis vegetans" were, he (Dr. Gray) considered, simply fungating granulomatous lesions, without vesiculation, bullous formation, or pustulation.

Dr. H. G. ADAMSON said he felt that a point against the diagnosis of pemphigus vegetans was the fact that no bullæ had developed after the patient had taken iodide of potassium. Patients with pemphigus were very intolerant of iodides and even small doses of iodides led to fresh crops of bullæ. He recalled a case with condyloma-like fungating lesions in the flexures, which remained undiagnosed until the patient was given iodides, when bullæ at once appeared and the patient eventually died from pemphigus vegetans. In two other cases, one fatal, the onset of pemphigus vulgaris dated from a local application of tincture of iodine.

Dr. DORE (in reply) said that the patient stated that she had had no medicine at all before the lesions began. As regards the other patient mentioned, she was under Dr. MacCormac, and was ill when she entered the Middlesex Hospital, but there had been no constitutional symptoms since. She was first admitted to the Marylebone Infirmary two years ago.

Case for Diagnosis.

By A. M. H. GRAY, C.B.E., M.D.

THE patient, a woman aged 51, first noticed the present eruption about two years ago. It began as a patch with "blocked pores" on the left shoulder, and this patch gradually increased in size till it attained its present dimensions. Since then other patches have appeared. There was a good deal of itching, but this has subsided as a result of X-ray treatment.

Present Condition.—The following lesions are present: (1) A large circinate patch on the left shoulder, extending slightly to the right of the middle line from the root of the neck to the fourth dorsal spine, below, to below the left scapular angle and across the deltoid to the third left rib, and in front over the episternal notch along the anterior border of the left sterno-mastoid muscle. The posterior edge shows the character of the active eruption best. It consists of some six or eight rows of follicular papules, slightly reddened, and each containing a large, horny plug. The edge is about ½ in. in width. The same condition prevails, but to a less marked degree, around the remainder of the edge. Within this margin the skin is for the most part of normal colour but it is studded with numerous pin-head sized scars. There are, however, several erythemato-squamous patches within this area; these have a coppery, rather transparent appearance, and the scales are large, thin and papery; they have a softish infiltration. Some linear reddish streaks, rather like early striæ atrophicæ, are also present.

(2) A patch of the same character but less marked, and about 4 in. in diameter, is present on the right shoulder and pectoral region.

(3) Two or three scattered erythemato-squamous patches are also present on the left arm below the deltoid insertion.

The only other lesion is an exfoliated patch on the left side of the tongue, about the size of a hazel-nut.

There are no physical signs of any general disease.

I was first inclined to diagnose syphilis, but the Wassermann reaction was negative. I then considered the question of lichen planus (lichen plano-pilaris) or early pityriasis rubra pilaris. As far as the last-named is concerned, I have never seen an asymmetrical localized pityriasis rubra pilaris, but I think patchy types have been described. The spines are too large for it to be pityriasis rubra pilaris, in which disease also scarring does not occur. The erythematous patches have no resemblance to the erythematous areas in this disease, nor do the lesions closely resemble those of lichen planus.

She was a private patient originally, living in the country. I saw her once a year ago, and tried ordinary salicylic acid ointment. Latterly, I have sent her into hospital, and in order to stop the itching she has had small doses of X-rays. This treatment, however, has not had much effect on the erythematous patches, or on the spiny lesions.

Discussion.—Dr. J. A. DRAKE suggested that the condition was lichen planus. This disease was deceptive and elusive. There was the peculiar bluish purple colour so often seen with lichen planus, and there was also the association of follicular papules.

Dr. J. M. H. MACLEOD agreed with Dr. Drake; there was a pink tint, and definite atrophy, particularly in the patch on the arm. These appearances suggested lichen planus.

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Case for Diagnosis.

By M. SYDNEY THOMSON, M.D.

PATIENT, a female, aged 44, first came to hospital ten days ago and stated that the eruption began on the right knee last November. Very definite soreness was immediately followed by an even more painful blister. To-day this lesion is roughly circular and about the size of a crown piece. The surface is slightly excoriated, whilst the whole plaque is appreciably raised above the level of the surrounding skin, bluish red, slightly infiltrated, and obviously somewhat œdematous both in the epidermis and dermis. Towards the upper edge there are three small nodules situated superficially in the true skin and attached to the epidermis, the largest of them having a diameter of $\frac{1}{3}$ in. On each elbow the appearances are much the same except that the lesion on the left shows less advanced changes. On the right elbow there is a plaque similar to that on the knee, but only of the size of a sixpence. Its centre is surmounted by a bulla extending almost to the edges of the lesion and containing faintly opalescent serum and blood. These areas are all exquisitely tender even to the lightest touch. On the legs the lesions are more polymorphic, and cover roughly the lower third of the posterior aspects of the limbs. Over the tendo Achillis there are irregularly linear brownish-blue patches $1\frac{1}{2}$ in. in length, infiltrated, and covered by some hyperkeratosis with follicular "pumice-stone" holes. On the right side there are also three small nodules similar to those on the knee. Each is faintly yellow in colour and tough. Clinically, it is difficult to determine whether this is due to intense œdema, to cellular infiltration or to something even more solid. Scattered irregularly about are numerous small papules, most of which are follicular, and all seem to be slightly infiltrated, whilst the colour varies from dull red to a brownish yellow.

The section is from a piece of material taken from the leg. It demonstrates a patchy infiltration of the dermis and papillary layer. Here and there invasion of the epidermis has occurred to a slight extent, the cells being mostly lymphocytes with some plasma cells and polymorphonuclears. There is slight œdema of the papillary layer and epidermis, together with a few areas of cell necrosis and rare tiny epidermal abscesses. Slight hyperkeratosis and parakeratosis. The infiltration is very markedly perivascular. Special staining for organisms, elastic tissues, etc., gives no further assistance.

Wassermann (blood) negative. Blood-sugar normal, but there is a blood-cholesterol of 263 mgm. per 100 c.c. This would seem to be of some importance as none of the following possible causative agents seem to be present: xanthoma, diabetes, chronic parenchymatous nephritis, obstructive jaundices and pregnancy.

Patient's past history and family history contain only one fact of possible importance. She suffered from headaches very severely until last November, and was treated for them by her doctor, but since that date she has taken a few aspirins only. It has been suggested that one of the halogens might be responsible for the eruption. This requires further investigation, but it does not seem probable, for when the test for indicanuria was made, there was no discoloration of the chloroform and no obvious odour either of iodine or of bromine.

Dr. A. M. H. GRAY said he regarded this as a mild example of a condition which he showed at the Section in April, 1913, and at the Seventeenth International Congress of Medicine. The lesions began as opaque white papules, and showed a tendency to develop

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central hæmorrhage. The larger lesions developed into rings with a raised white margin and pigmented centre. The lesions involved the limbs extensively, but picked out elbows, knees and ankles, and in these regions were much hypertrophied and almost warty. The larger lesions showed blistering, often hæmorrhagic. He described the case as "persistent erythematous eruption," and both Dr. Pringle and Dr. Adamson considered it belonged to the erythema multiforme group. Both Unna and Jadassohn saw it at the International Congress, and sections were inspected; the former thought it was a disease which should be separately described. In bore some similarity to the more acute cases of granuloma annulare. The description of his case appeared in the *British Journal of Dermatology*, 1913, xxv, 160.

Section of Electro-Therapeutics.

President—Dr. G. B. BATTEN.

The Growth of Tumour in Tissues Exposed to X-rays and Radium.

By S. RUSS, D.Sc., and G. M. SCOTT.

(Cancer Research Laboratories, the Middlesex Hospital.)

(ABSTRACT.)[1]

PREVIOUS experiments have shown that rats which have been exposed to repeated small doses of X-rays and afterwards inoculated with Jensen's rat sarcoma, do not grow the tumour quite so quickly as normal rats [1].

Murphy [2] found that after purely local irradiation of the skin of mice, *intra*-cutaneous inoculations of tumour rarely grew, but in the case of *sub*-cutaneous inoculations, there was very little difference between the growths in the treated mice and the controls.

As these observations have some bearing on the therapeutic uses of X-rays and radium, more experiments on much the same lines have been carried out, and from the data in the following paper it seems clearly indicated that implanted sarcoma of the rat does not grow at the normal rate in an area which has been given a suitable dose of radiation.

EXPERIMENTS 1, 2, 3, 4 AND 5.

Young rats were inoculated on the flank with small pieces of Jensen's rat sarcoma; three or four days later the graft was covered with a lead disc, 1 mm. thick and 9 mm. diameter, and the surrounding tissue exposed to a square radium capsule for 30, 45 or 50 minutes. At half-time it was turned through an angle of 45°, so that the irradiated area might be more or less circular. In some cases control tumours were grown in the same rats; in others in different rats; but in either case the control tumours were always the larger. The data are given in Table I. The capsule used was 1·8 cm. square, was screened by 0·2 mm. of silver and contained 30 mgm. $RaBr_2 2H_2O$.

A single exposure of 30 minutes produced epilation and permanent loss of pigment in the hair.

TABLE I.

	Length of Irradiation	Irradiation	Average volume experimental	Average volume controls	Ratio
30 mgm. capsule ...	30 minutes, once ...	After inoculation ...	21·0 ...	35·2 ...	0·6
,, ,, ...	30 minutes, twice ...	,, ,, ...	0·9 ...	16·6 ...	0·54
,, ,, ...	30 minutes, ,, ...	,, ,, ...	2·6 ...	13·2 ...	0·20
,, ,, ...	45 minutes, once ...	Before inoculation ...	0·4 ...	3·8 ...	0·10
,, ,, ...	50 minutes, ,, ...	After inoculation ...	10·4 ...	21·5 ...	0·48

EXPERIMENT 6.

In this case the capsule was screened with 1 mm. of aluminium and the length of the exposure increased to 60 minutes; the volume of the tumours in the irradiated areas was 0·52 that of the controls.

[1] This paper will be published in full in the *British Journal of Radiology* (B.I.R. Series).

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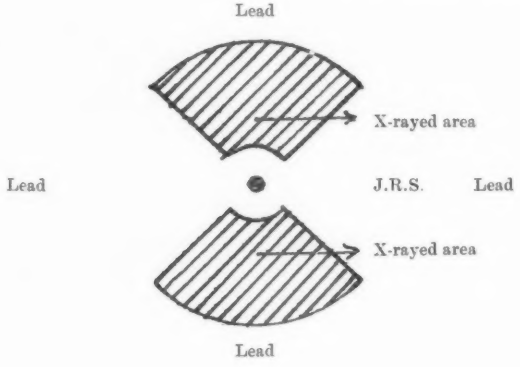
All these observations on more than 100 rats suggested that irradiation of the skin distinctly hindered the growth of the tumour, but this point was tested in a more exacting fashion.

Experiment 7, etc.

Each rat was covered with lead (2 mm. thick, enough to stop the X-rays used in these experiments) except for two areas, the actual size of which is given in the diagram.[1] The rats were then exposed to measured doses of X-rays while under an anæsthetic. In some cases they were inoculated with Jensen's rat sarcoma before the X-ray exposure; but in most cases this was done three or four days afterwards, as near as possible into the middle of the screened area directly between the two exposed sectors. Measurements were taken and charts made on squared paper twice a week;



six different series all show the same tendency, the Jensen rat sarcoma growing much more readily in the normal areas. In many cases where the irradiated area was eventually invaded by the tumour the growth was much flatter than in the normal regions.

Thirty minutes' exposure to X-rays,						subcutaneous inoculation three days later.
Forty-five	,,	,,	,,	,,	,,	,, ,,
Twenty	,,	,,	,,	,,	,,	three days before.
Thirty	,,	,,	,,	,,	,,	,, ,,
Forty-five	,,	,,	,,	,,	,,	one month later.

All the rats in these experiments were inoculated in the flank; normally Jensen's rat sarcoma in this position grows more or less spherically with perhaps a tendency to track into the axilla and towards the chest; one X-rayed sector was between the nodule and the axilla and the other directly below it. The tumour grew more readily towards the chest and avoided the X-rayed area near the axilla; so another batch of rats was inoculated and the X-rayed areas turned towards the chest and back. The results of this experiment show that there is exactly the same thing happening; the tumour growing more readily in the normal tissues than in the X-rayed, irrespective of the position.

The X-rays used in all these experiments were generated from a Coolidge tube, with an alternative spark gap of 5 cm. between spheres 5 cm. diameter. Under

[1] For the loan of the block illustrating this diagram the authors are indebted to the *British Journal of Radiology*.

the same conditions of output and distance, it takes about 50 to 55 minutes to give a lethal dose to Jensen's rat sarcoma. The effect of these doses (20, 30 and 45 minutes) on the skin varies from partial epilation to slight blistering.

REFERENCES.

[1] RUSS, CHAMBERS and SCOTT, *Proc. Roy. Soc.*, B., vol. 92, 1921. [2] MURPHY, *Journ. Exp. Med.*, 1921, *et seq.*

The Action of Radon "Seeds" on Tumour and Liver Cells of the Rat.

By S. RUSS, D.Sc., and G. M. SCOTT.[1]

(Cancer Research Laboratories, the Middlesex Hospital.)

(ABSTRACT.)

THE term "radon 'seed'" is used for describing a small glass tube, 3 to 4 mm. long, filled with radium emanation. The external diameter is about 0·5 mm., and the internal 0·3 mm.; the glass is so thin that besides letting through all the gamma rays, it lets through a large proportion of the beta rays as well.

Failla has published data on the distribution of radiation from seeds, and Cutler has shown their effect on the muscles of the rabbit.

For the experiments described in this paper, Jensen's rat sarcoma and normal rat liver have been used. Liver was found to be easy to deal with and more satisfactory than spleen.

The seeds were inserted vertically into the surface of the liver, leaving the top point just visible. They were left in for various times, and sections of the liver were taken, either when the seed was taken out or some days later.

The same technique was used when dealing with the Jensen's rat sarcoma, but owing to the growth of the tumour, there was often difficulty in finding the seeds. In some cases a dummy seed (one holding no radon) was put in to mark the place where the active one was taken out, but this was not always a success.

As control experiments dummy seeds were put into the liver for times ranging from a few hours to ten days; apparently these had very little effect on the surrounding tissue.

The first series of experiments on the liver with 1 mc. seeds showed a gradual increase in the area affected, as the exposures were lengthened from half an hour to ten days; this could be seen with the naked eye. Microscopically, the most striking feature was the sharp line of demarcation between the damaged and healthy-looking cells; there was, however, a small intermediate region, and reference is made to this later.

One of the chief points of interest was whether this abrupt transition was dependent on the *intensity* of the *radiation* or on the *energy* absorbed.

A section of liver which had been exposed to a 1 mc. seed for two hours was chosen as a fair specimen to work upon. A transverse section across the middle of the seed showed that the sharp dividing line began just under 1 mm. away; beyond this distance the liver cells appeared to be very little affected by the radiation, judged by their staining properties. The question was, had these cells been able to preserve their equilibrium because the intensity of the radiation (owing to distance and absorption) had fallen, or was it due merely to the smaller dose (i.e. energy absorbed)? To test this point a 1 mc. seed at full strength was put in a rat's liver for two hours and a section taken forty-eight hours afterwards.

[1] This paper is published in full in the *British Journal of Radiology* (B.I.R. Series), July 1927, p. 239.

This seed was kept until its intensity had dropped to about 10 per cent. of its original value and then used again ; this time it was left in for $22\frac{1}{2}$ hours, so that the actual dose of irradiation was the same as in the original two-hour experiment. This was calculated from $I^1(i - e^{-\lambda T}) = \lambda I t$; (I and I^1 are the strong and weak intensities). Later on the same seed was used when it had fallen to 2 per cent. of its initial strength, the length of exposure being prolonged proportionately. After this the seed was used again when its intensity had fallen to about one per cent. of its original value ; this time it was left in for thirty days, making the dose still equivalent to the original two hours. All the sections were taken forty-eight hours after the end of the exposures.

The range of damage varied very little in the first three experiments, but when the strength of the seed had fallen to 0·01 m.curie, the zone was appreciably smaller, although the cells were given the same actual physical dose.

Calculation shows that if the intensity of the radiation received by a region the size of the nucleus of the cell falls below the rate of about one beta particle per second, the tissue is apparently undamaged. Provided this intensity is exceeded, the effect on the cells depends on the quantity of energy absorbed.

Measurements of the destroyed areas of the livers exposed to 1 mc. seeds for half an hour, four hours, etc., showed that up to about eight hours the zone of destruction was proportionate to the actual dose, but this did not hold for the long exposures.

The experiments with tumours were carried out in the same way, but it was much more difficult to get satisfactory specimens ; a rapidly-growing tumour had to be used in order to obtain uniformity in the tumour, which often made it very difficult to find the seed without damaging the surrounding tissue. By experimenting with a large number of tumours, it was possible to see that the effect was practically the same as on the liver ; a fairly sharp line divided the damaged from the apparently healthy cells, and the size of this damaged area depended on the actual dose and not on the intensity of the radiation.

Microscopic Observations.

Another striking feature in addition to the sharp line of demarcation between the damaged and the apparently healthy cells is the diminished effect which the radiation has on the cells directly round blood-vessels. Cells so situated appear well stained while others at the same distance from the seed are severely damaged. This effect can be seen in practically all the sections.

The conclusion seems justified that liver cells near blood-vessels are not so easily damaged by radiation as others ; if this holds for human cells, it would certainly be an argument for reducing the blood-supply of tumours when possible during therapeutic exposures to X-rays or radium.

The demarcation line between severely damaged and normally stained cells in some cases is not so sharp, and there seems to be a zone of cells somewhat affected by the radiation.

Professor McIntosh has been good enough to look at some sections and give his opinion as to where the dividing lines should be drawn. Taking a specimen of liver which has been irradiated by a 1 mc. seed for ten days, he suggests the following divisions :—

1. Zone of complete destruction ... ... radius 2·7 mm.
2. Width of zone between surviving leucocytes, fibroblasts and first surviving liver cells ... 0·2 mm.
3. Width of zone between partially destroyed liver cells and complete survival ... ... 0·3 mm.

From these figures it will be seen that there is a distance of 0·5 mm. between the " complete destruction " zone and the edge of the " complete survival " area. The

dose of radiation would be reduced by the increased distance and by the absorption These are both easily measurable. By the distance factor the radiation would be reduced to 77 per cent., and by absorption to 83 per cent., so that the dose actually received by the "completely surviving cells" would be not more than 64 per cent of that received by the "completely destroyed" ones, 0·5 mm. nearer the seed.

It is difficult to get such accurate measurements with the tumours owing to the difficulties before mentioned, but from the general similarity of the size of the zones of destruction in the liver and tumour, it is probable that a similar ratio holds good for the tumour.

Previous experiments on lethal doses with beta, gamma and X-rays have shown that although 25 per cent. to 30 per cent less than a lethal dose does not kill the tumour cells, their subsequent growth is very much slower.

Conclusions.

(1) Limited regions of destruction are caused by inserting radon "seeds" into tumours and livers of rats.

(2) The extent of the damage depends much more on the amount of energy absorbed than on the actual intensity of radiation.

(3) Cells around blood-vessels appear to be protected to some extent from the damaging effects of the radiation.

References.

Failla, *American Journal of Röntgenology*, vols. xv and xvi, 1926. Cutler, *American Journal of Röntgenology*, vol. xvi, 1926.

Dr. N. Finzi said that he had some difficulty in being convinced by the experiments where two sections of an animal had been irradiated and then the implantation made between them. It would be noted that in practically every instance the irradiated area was invaded, and, although the tumour showed a tendency to grow rather less on this area, if there were any considerable effect it should not have grown at all in the irradiated regions. So many factors, however, entered into these experiments that it was very difficult to disentangle the meaning of the results.

With regard to the second paper. Seed implantation had been introduced by Stevenson and Joly of Dublin, but, after their original papers, very little had been published by them. The work was carried on by Janeway, in America, and by Burroughs, in Manchester. Regaud, in Paris, also tried it. It was soon found, however, that so much sloughing was caused that the results were not satisfactory. Regaud began the use of seeds or needles covered with a platinum filter, which at first was 0·3 mm. and was then increased to 0·4 mm. and 0·5 mm.; he found that very much better results were obtained with these filtered foci. In America, also, they seemed to have given up the use of unfiltered seeds—the Mayo Clinic, for example, were using a gold filter of 0·5 mm. He thought that the experiments outlined in the paper would be of more value to the clinician if performed with filtered seeds. He hoped, therefore, that Professor Russ would carry out a new series, using filters.

Section of Epidemiology and State Medicine.

President—Dr. S. Monckton Copeman, F.R.S.

The Prophylaxis of Measles with a Suggested Scheme for Dealing with Epidemics.

By W. S. C. Copeman, M.B., M.R.C.P.

The Infectivity of, and Mortality due to Measles.

MacNalty [1] in his report to the Local Government Board in 1918 stated that "The disease is so universal in this country that few children escape it," whilst Butler [2] (1913) found, as the result of his study of the statistics for the public elementary schools of Willesden, that amongst persons who had attained the age of 15 and upwards, only 2·7 per cent. had escaped the ravages of this disease. That the severity of measles has, in the past, been grossly underrated will be generally admitted, and the recent action of the Metropolitan Asylums Board in giving it preference over scarlet fever in the wards of their hospitals tends to confirm the truth of this statement.

The Office International d'Hygiène Publique gives the death-rate from measles as 27·05 to 37·5 per cent. of deaths caused by all the other infectious fevers during an average of twenty years. During the outbreak in Paris in 1922-23, in the Children's Hospital the death-rate was 14·93 per cent. This was the lowest death-rate in any of the hospitals in Paris. In England the death-rate from this cause in all M.A.B. institutions in 1912 was:—

Under 1 year	...	20·3 per cent.
From 1-2 years	...	18·8 per cent.
From 2-3 years	...	8·0 per cent.

In view of the enormous wastage of child life from this source, it would appear to be desirable to institute more efficient prophylaxis for this disease if such be attainable; since it is obvious that methods which may be effective in the case of other of the exanthemata, have not the same efficiency when applied to measles, owing to its long period of incubation, and its four days of invasion, during which period the patient is in his period of maximum infectivity, but during which it is often undiagnosed. It is unfortunate that epidemics of whooping-cough and measles do not provoke the same official or public interest as do those of diphtheria, scarlet fever, or smallpox, although often they are much more fatal.

Present Methods of Prophylaxis.

These methods are more or less traditional, and results are not really expected; they are not rigorously carried out; they consist for the most part in isolating suspected cases and contacts for a period of fifteen to twenty-one days. Further, the class or school of which the victim was a member is closed down nine days after the sickening of the first child for five days, during which time all those who are incubating the disease will then develop it at home.

Wheaton and MacNalty further recommended:—

(1) Compulsory notification.
(2) Early visit to the patient's home by health visitor.
(3) Provision of adequate nursing and medical attendance in patient's own home.

All these measures, which, as results show, are at present scarcely adequate in themselves, would, if some method of producing immunity in those susceptible were added to them, help in the control of those epidemics which experience shows us we may expect every few years. I shall return later to the subject of the control of these epidemics.

Serological Methods: Historical.

Francis Home (1719 to 1813) is known to have made use of a form of active immunization against measles in 1759. Details as to its success or otherwise are, however, lacking. Hektoën, in 1905, seems to have produced a mild form of the disease.

Some forty years ago, so I am informed by Dr. A. K. Chalmers, the Faculty of Physicians and Surgeons of Glasgow appointed a vaccinator, by name Hugh Thompson, to whom the parallel of vaccination for measles appealed so strongly that he was in the habit of blistering measles convalescents and vaccinating contacts with the serum. He claimed good results, but was apparently not universally believed.

The true history of serological prophylaxis for measles starts in 1916, when Anderson and Goldberger showed that an injection of blood from an early case of measles into a monkey assured its immunity against further inoculations, indicating that antibodies were formed. This inspired Nicolle and Conseil to inject the serum from a human convalescent case during a small epidemic at Tunis in 1916. Their success proved the starting point for all subsequent work on the subject. In the same year Park and Zingher obtained good results during an epidemic on Welfare Island.

In 1918 Richardson and Connor [4], of Boston, U.S.A., produced active immunity by means of vaccination with the naso-pharyngeal secretion of active cases, combined with the injection of large amounts of convalescent serum; whilst in June, 1919, Degkwitz [5], working in Pfaundler's clinic in Munich, began his interesting trial of the method on a large scale, succeeding thereby in reducing the infant mortality under 2½ years by more than 70 per cent. No experimental work has, so far as I am aware, yet been published in this country, although I believe that a small trial of the method has been carried out in one of the northern fever hospitals,[1] and Miller and Smith [6], of Harrogate, read a paper before the Pathological Society of Great Britain and Ireland, early this year, on the results obtained in a small epidemic which they treated successfully by the method of "sero-attenuation."

Methods of Employing Serum.

The serum of convalescent cases of measles, and, to a less degree, that of adults who have had the disease during childhood, may be employed in five ways, the end-result achieved being in each case different. In cases of emergency, citrated whole blood may be used in place of serum, although larger quantities have, in this case, to be employed.

(1) It may be injected into a child who is not yet infected, a passive immunity lasting about one month being produced—"Sero-prevention." As, however, in the case of patients who have received diphtheria antitoxin, the acquirement by these subjects of active immunity later is considerably impeded. If the serum be injected during the first five days of the incubation period, passive immunity is still produced, and measles does not develop. The dose in this case will, however, need to be considerably increased, whilst after the fifth or sixth date of the incubation period no amount of serum will prevent the development of the disease, although its form will be modified.

[1] Since this paper was written Benson and Lawrie have published an interesting account of a small epidemic treated with success in the Edinburgh City Fever Hospital (*Edin. Med. Journ.*, April, 1927).

Degkwitz gives the following doses as being those suitable for a child of three years old:—

First to fourth day 2·5 will prevent measles.

Fourth to fifth day. Result uncertain even with large doses.

Seventh day. Even 40 to 50 c.c. serum will not prevent measles developing.

These doses are, however, in my experience [10] too small, and although, in view of the difficulty of collecting sufficient quantities of convalescent serum, it is obviously desirable to reduce the dosage to an effective minimum, most of the failures recorded in the literature would appear to be, in all probability, due to insufficient dosage.

Debré recommends the following dosage for producing passive immunity:—

Large children and adults, 6 to 8 c.c.

Children 3 to 10 years old, 3 to 6 c.c.

Children under 3 years, 3 c.c.

Park and Freeman suggest [7] as the result of observations on over 1,500 children in New York:—

Large children, 6 to 10 c.c.

Children under 3 years, 6 c.c.

Wulff [8], working at the Blegdams Fever Hospital in Copenhagen, states that it is difficult to determine the minimum effective dose, as it is probable that both the potency of the serum, and the reaction of the patients to it, vary considerably. He uses, however:—

Children over 4 years, 15 c.c.

Children over 1 to 4 years, 10 c.c.

Children under 1 year, 5 c.c.

All these authors prefer the unfiltered serum, which is, they say, superior in potency to the filtered.

(2) The serum may be injected into contacts after the sixth day of the incubation period. In this case measles will, in all probability, develop, but its course will be modified; this will produce, later, an active immunity ("sero-attenuation"). This is the method of election in all but certain circumstances, and will be referred to again later.

(3) It may be injected still later; at the commencement of the period of invasion. In this case the course of the disease will be unaffected, but an area of local blanching of the rash at the site of injection (phenomenon of Debré) will occur.

(4) Again it may be injected at even a later period than this; when the symptoms of the disease have already declared themselves ("sero-therapy"). In this case it will generally have no apparent action; the disease running its course normally.

(5) Serum may be injected into the unaffected subject, who is subsequently inoculated with the virus ("sero-vaccination"). In this case an active immunity will develop. This is probably the natural mechanism in method (2), when a patient who has received an injection of serum, afterwards becomes infected with measles.

Collection and Preservation of Serum.

This may, in some cases, be undertaken by the practitioner for use in his own practice, but its use on a large scale, in time of epidemic, must obviously necessitate centralized control. I shall return to this question further on.

The essential points to be observed, according to Debré [9], whose technique is simpler than that of Zingher, are as follows;—

(1) The donor should be a child (or adult) over the age of ten years, who should have had an unmistakable and first attack of measles. From 500 to 600 c.c. may be taken from adults.

(2) Convalescence should have been uncomplicated and the defervescence complete.

(3) The donor must be shown by test to be free from syphilis, malaria and tuberculosis.

(4) He should be kept under observation for several days after the blood has been taken, in order to make sure that he was not at the time in the incubation period of any blood-borne disease such as typhoid.

(5) The blood should be taken between the sixth and ninth day after defervescence, since this is the period during which experience shows the blood to be most rich in antibodies.

(6) The blood should be allowed to coagulate at room temperature, the serum being decanted aseptically later, and tested for sterility. It should then be pooled with the serum

from at least two other subjects, in order to standardize its potency as much as possible, and placed in ampoules containing 3 to 5 c.c., ready for use.

(7) The serum should be kept in the ice chest for four or five days before use, in order to allow time for the death of certain micro-organisms such as the *Spirochæta pallida*, which might, theoretically, be contained in it, in spite of negative tests.

(8) If any doubt as to its absolute sterility exists, it may be heated to 56° C. twice, at twenty-four hourly intervals (Nobécourt and Paraf). Degkwitz adds one drop of 5 per cent. carbolic acid for every 10 c.c. of serum.

(9) The serum, if kept in an ice chest, retains its activity for several months.

Degkwitz has recently reported on the use of a similar prophylactic serum prepared from sheep, whilst Tunnicliff and Hoyne state that they have managed to immunize goats by intravenous injections of green-producing measles diplococci and their filtrates.

Sir George Newman, in his annual report for 1925 [12], stated that the Ministry of Health were, in conjunction with the Medical Research Council, closely watching the investigations of Degkwitz. So far as I am aware, Sir George has not yet expressed his impressions of this work, but if one may judge from the sporadic literature which has appeared on the subject from other sources, since the issue of his report, the serum of convalescent patients still holds its own as the method *par excellence*, the sheep's serum merely producing, according to Progulski and Redlich [13], an inhibitory action on the appearance of the symptoms of measles, so that the incubation period is prolonged. It has caused serum sickness in many cases. Tunnicliff and Hoyne, however, record good results with the use of their serum.

Active and Passive Immunity.

Since the early days of serum prophylaxis, scientific opinion has gradually been turning from the desirability of producing a passive immunity in time of danger, to that of producing an active immunity at an earlier period. It will be remembered that one of the methods of employing serum described above ("sero-attenuation") has the double merit of being applicable during an epidemic, and yet of producing active immunity. This is the method of election of Debré and others. If the serum be injected after the sixth day and before the ninth day, measles will develop, but the attack will be modified in the length of its invasion period, and by the absence of catarrhal symptoms, Koplik spots, and high temperature. The eruption also, if present, is less confluent, and the macules are smaller. Complications do not apparently occur. Passive immunity is only preferable in the case of children under three years of age, or of those very debilitated by tuberculosis or other disease, since 70 per cent. of all deaths from measles occurs in children under this age; children over this age who are in robust health seldom die.

Results obtained.

The majority of the results published have tended to show that the method of sero-prophylaxis has a real value applied to measles. Zingher stated before the Academy of Medicine in 1924 that "Convalescent measles serum, plasma, or whole blood has a definite value in the prophylaxis of measles." Debré and Joannon reported the case of an asylum for ailing children at Médan (France) where the mortality-rate averaged 70 per cent. of the infants attacked. Subsequently, however, two commencing epidemics were, by the employment of serum obtained from Paris, entirely wiped out, much to the surprise of the staff, without death occurring. Degkwitz and his assistants, working in Munich, have practised over 2,000 successful injections with the serum of convalescents. Park and Freeman reported recently [14] on the successful application of the method in over 1,500 cases, whilst Haas and Blum [15] confirm its value with, however, a smaller number of cases. My personal

experience, which I have recorded elsewhere, also tends to do so. Toomey used the method last year at the Cleveland City Hospital, and found that in many cases serum which had been kept (on ice) for as long as twelve months seemed still to be potent.

Conclusions.

From what has been said above, it would appear to be legitimate to state:—

(1) That the serum or blood of persons who have, in the past, suffered from, or are convalescent from measles, has a definite prophylactic value in this disease.

(2) That if it be injected in sufficient dose, during the first five or six days after infection it will produce a passive immunity which will last for some weeks; whilst if injected between the fifth and ninth day of the incubation period, a modified form of the disease will develop, which in turn leads to the establishment of an active immunity which some observers consider to be permanent.

(3) That in the absence of certain contra-indications, which have been mentioned, it is better to allow an attenuated form of the disease to develop ("sero-attenuation") than to rely on renewing the patients' passive immunity in time of danger by means of serum.

(4) The use of convalescent serum in this latter way ("sero-prevention") should be limited, normally, to children under three years of age, since these supply 70 per cent. of the measles mortality. In this way the disease may be warded off until the child is of an age when measles is no longer so dangerous. It is also indicated in the case of children suffering with tuberculosis, rickets, diphtheria, whooping-cough, and in feeble children, in very cold weather, and in institutions.

(5) The prophylactic dose of serum has to be increased in direct ratio to the time of exposure to the disease.

A Suggested Scheme for Dealing with Measles Epidemics.

It is not pretended that the following scheme, which is based on the creation of a centre for the distribution of prophylactic serum, is original. It is a *pot-pourri* of the writings and ideas of many workers, notably of Debré, and Godfrey.

The Académie de Médecine of Paris recommended in 1925, largely as the result of Debré's work, that a centre should be established for the collection and storage of serum from cases of measles and whooping-cough. The Minister of Health, M. Justin Godard, formed a commission to consider this recommendation; the result of their deliberations was that two centres were established. At the centre to which I had the good fortune to be attached for a time (at the Hôpital des Enfants Malades) in the first five months, 10 litres of serum, or 2,000 protective doses, were collected. This was sufficient for all needs.

Our aim at present, in England, is to prevent every case of measles. Our measure of success may be judged by the fact that about 90 per cent. of adults have suffered from the disease. In the light of present-day epidemiological knowledge, it appears, as a matter of fact, to be a policy of doubtful wisdom to endeavour to do so, unless adequate artificial immunization were insisted upon, since sooner or later the infection would return, and finding virgin soil, the tragedy of the Faröe or the Fiji Islands would be repeated on an even larger scale. The deadly virulence of the disease in the absence of any immunity was seen, I believe, during the War, amongst the first American troops from the Middle West.

P.-L. Marie has tersely stated that: "le but n'est pas tant de faire disparaître la maladie de la face du monde, que de reculer son apparition jusqu'à un âge où les risques qu'elle fait courir deviennent très minimes."

This sums up the outlook towards the disease which should, I consider, become general in this country. We should endeavour, by means of efficient organization, with clear-cut aims and methods, to *control* the disease, rather than to stamp it out. It has already been stated that 70 per cent. of all deaths from measles occur under the

age of three years. It is, therefore, during these fatal first three years that we must eliminate the disease; postponing it, if necessary, until the child is over this age and therefore more able to combat it. Few healthy children over five years of age die from measles. It has been well said that measles attacks without distinction, but kills with discrimination. If we can accomplish this control, a great step forward will have been taken. We shall have reduced the mortality, if not the morbidity, of measles.

The methods by which it would become possible to achieve this aim are twofold. First, a skeleton corps of trained public health workers, who would have the power in time of need to call upon less specifically trained workers in allied departments, such as welfare centres, since the need would be for a large body of workers for a short time. Secondly, a knowledge of the whereabouts of the children under three years whom it is desired to protect. I shall deal with both points shortly.

It may here be well, before going into further detail, to consider what alternative methods we have of dealing with an epidemic of measles.

(*a*) The present method, by which we aim at preventing all cases. This, as has been pointed out, is probably fundamentally unsound, and would, if successful for the time being, ultimately increase the death-rate.

(*b*) To do nothing, and trust to spontaneous measures in prevention of undue mortality. This is bad in so far that it creates the impression in the minds of the public that measles is an unimportant disease, and, since the disease would not be notifiable, we could have but little information with reference to the progress of the epidemic apart from the death-rate.

(*c*) This is the method which has already been outlined.

It was stated above that 70 per cent. of all deaths from measles occurs in children under three years old. Now (according to Godfrey), the total population at this age is only 6 per cent. of the total population, whilst the actual cases of measles occurring during this period are only 12 to 15 per cent. of the total cases (in New York State).

It will be seen, therefore, that if we concentrate (from the public health point of view) only on this 12 to 15 per cent. of all the occurring cases, the organization needed will, in all probability, not be greater than that required under the present system. Medical and nursing care might be provided for the severe older cases, as is done at present in the M.A.B. Hospitals.

This system would obviously be easier of application in large towns than in smaller places, and since measles appears to be more fatal in these areas, apart from the higher case morbidity, this is to some extent a provision on the part of Providence. Compulsory notification would have to be instituted.

It is suggested that a serum-collecting station, comprising a small laboratory and waiting room, and preferably attached to some institution where measles cases are available, should form the centre of every unit of the scheme; a unit would in most cases comprise a large town, and the M.O.H. would be the central authority for the unit. The personnel of the centre would form the "skeleton corps" of trained men who would, in time of epidemic, take control of it under the supervision of the M.O.H. Between epidemics they would collect a store of both adult and convalescent serum as opportunity arose, and work on the following lines:—

(1) To educate the parents of small children in the belief that epidemics can roughly be predicted; thereby preparing the ground for a future campaign.

(2) To educate parents in the belief that measles *in children under three* is DANGEROUS, and the younger the child the more dangerous the attack, and that it is dangerous at any age if the child be weakly.

They must beware of saying that "measles is dangerous" without specifying "under three," since the public know that in older children it is not particularly dangerous, and think the propaganda is merely for the benefit of the doctors.

(3) To make it known that serum is available for these cases, but that its amount is

limited. That donors (voluntary or compensated) are continually required to "save the children."

This personnel could visit the scene of epidemics and deal with them on the spot, on application from general practitioners. They need not limit their activities to measles, although this would be advisable in the initial stages.

The chief available channels for this propaganda are :—

(1) The newspapers. The local or general press are generally willing to insert articles and appeals; provided that they can be given some news value. This can be done. All deaths under three could be reported, and statements that the blood of the parents may save the little one can be given dramatic interest. Epidemics could be predicted.

(2) Private doctors, who would be told of the scheme, and would verify it on demand from patients.

(3) Occasional public lectures on more or less popular lines.

(4) Notices, such as are displayed in some boroughs, notably Holborn (*re* diphtheria and flies).

These may be posted :—

(*a*) In the streets, etc.
(*b*) In Infant Welfare Centres.
(*c*) In Hospital Out-Patients' Departments.
(*d*) In Maternity and Municipal Hospitals and Clinics.

In addition, the staff concerned should have at their disposal a card index, containing the names and addresses of all children under three years of age in their area. This could be managed easily if duplicated copies of birth certificates reaching the M.O.H. were sent to them. In this way probably more than 90 per cent. of the true number of births would be accounted for. The names of the children could be indexed by streets or districts, and all notifications and child death certificates would be sent through this department. The names of cases of children on their index thus known to have died or had measles would be removed. The index would be revised every year by the removal of all those cards relating to children who were then more than three years old.

In conclusion, I will quote from a presidential address delivered by the President of our Section on the occasion of the Congress at Bristol, last year, of the Royal Institute of Public Health [17]. He said: "In view of the importance of lessening, if possible, the mortality from measles in this country, you will doubtless agree that it is eminently desirable that a thorough investigation of the whole subject should be undertaken as speedily as possible, somewhat on the lines of that recently initiated by the Departmental Committee (of the Ministry of Health) in the case of scarlet fever."

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Discussion.—Dr. S. MONCKTON COPEMAN, F.R.S. (President), said that the fact that the author the paper was his son made effective criticism of the paper somewhat difficult. The subject of measles prevention was one to which he had referred in his presidential address on immunization against certain infectious diseases. But, unlike his son, he had not any actual personal acquaintance with the more or less experimental methods now being employed to a small extent in this country. In that paper, Dr. William Copeman had not given any details of his own work in this direction, when, about a year ago, acting as Medical Officer at the Children's Hospital, Paris. The results obtained there, at a time when measles were very prevalent in Paris, had been decidedly satisfactory. The present methods of using "convalescent serum" for the purpose of inducing immunity was, necessarily, a somewhat crude one, but until such time as we knew as much about the actual specific micro-organism concerned as in the case of diphtheria (and perhaps in scarlet fever also), not much advance beyond the present methods could be anticipated. In America, Ferry and Fisher had indeed claimed not only to have isolated the specific microbe, but also to have produced an effective toxin. The latter, they maintained, could be utilized as in the Schick and Dick tests for diphtheria and scarlet fever respectively, and also an antitoxin for immunization purposes. But so far as he was aware this work had not, as yet, been confirmed by other observers.

Sir GEORGE BUCHANAN said he found it difficult to accept the statement that the aim of practical epidemiologists in England at present was to prevent every case of measles. He had never, personally, based any action on this objective or recommended such action. On the contrary, it was necessary to accept the evidence that on the basis of all experience up to now the great majority of the population would have measles at one age or another. The advantage of postponing attack until adult life was by no means clear, and good evidence of this was furnished in many cases during the war. He cited particularly the severity and fatality of measles in the Highland Brigade when brought down to Bedford in 1914, and of course, there were the classical cases of measles in isolated communities such as the Faroe Islands and in Fiji. In the latter islands he understood from Dr. Montague that it was now realized that there was little wisdom in attempts to exclude the importation of measles, and that it was better to allow the population to acquire its immunity in the ordinary way by contracting the disease in childhood.

There was a special reason for trying to prevent children under three from taking measles, in view of the much higher mortality rate at that period. This was not necessarily a factor of age since children in good circumstances, treated with good nursing, rarely succumbed. Nevertheless, action concentrated on protection of this age-group from infection was in general justifiable and useful, especially if it formed part of systematic methods to secure better treatment of the actual cases in hospital or at home. Under our present system universal compulsory notification was not necessary for this purpose. Any district which organized such measures to deal with measles could obtain powers without difficulty, for local notification, and in a district where there was no such organization, mere notification was of little use.

The paper related the possibility of an additional means of securing immunity without serious risk, namely, by a serum treatment which would diminish the virulence of the attack in a child who had already been infected. Much could be said in favour of a system which produced this result, whether in individual special instances or as a part of a scheme for a community. Its trial in any case did not seem to call for any preliminary general measures such as universal notification of measles or an intensive general propaganda. As a practical matter he (the speaker) doubted whether many parents would accept the inoculation of a healthy child contact with serum from measles convalescents. The dangers might be infinitesimal, but it was a matter on which prejudice would certainly be strong, much more so than in the case of an animal serum. Intensive general propaganda on questions like these was a weapon which ought to be used with considerable caution, and he hardly considered that the preventive treatment outlined had reached a stage in which such propaganda could be justified.

Sir WILLIAM HAMER called attention to the great fall during the last ten years in mortality from measles in London, which had been associated with co-ordinated efforts made to secure early treatment, either in hospitals (mainly those of the Metropolitan Asylums Board) or, alternatively, with the help made available by nursing associations in the children's homes. It had been said that decline in the birth-rate was partly responsible, but he would refer the

critically minded to the chart on p. 101 of the Annual Report of the County Medical Officer (London) for 1925, which showed that mortality in the first five years of life (per 1,000 children born in each year) subsequent to 1916, had only just exceeded *one half* that of the average for those born in earlier years. This remarkable result might be considerably improved upon, if still further efforts were made to secure earlier treatment for children whose home conditions were not such as to facilitate medical and nursing care.

Dr. M. B. ARNOLD said that it would be interesting to consider how the diseases named in the Notification Act of 1889, which, of course, did not include measles, came to be chosen. There had been, on the whole, a remarkably greater fall in mortality from the notifiable than from the non-notifiable infectious diseases. There certainly had been a time when many medical officers of health thought the notifiable diseases were the only diseases with which they should ordinarily be concerned and an outbreak of measles did not greatly stir the public health office.

Dr. J. D. ROLLESTON said that Dr. Copeman had not only given an excellent review of the literature but had also shown that he had a practical knowledge of the method he recommended. He (Dr. Rolleston) had not had any personal experience of the serum prophylaxis of measles, but he could claim a fairly wide literary acquaintance with the method, to which he had drawn attention in the *Medical Annual* each year since 1920, as well as in *Medical Science: Abstracts and Reviews* in 1922 and 1923. The meeting had heard all the benefits to be derived from the method. Were there any dangers or drawbacks attached to it? In spite of the very large scale on which it had been carried out, only three deaths, so far as he knew, had been reported, namely, two by Vassal [1] and one by Schlossmann,[2] and the only other untoward occurrence was tuberculous infection of the site of injection and corresponding lymphatic glands following inoculation of serum from measles patients who had a positive von Pirquet reaction but no clinical evidence of tuberculosis (Kundratitz[3]). The chief drawback to the method was the difficulty in obtaining convalescent serum, even when rewards were offered to donors. In large towns such as Paris and Brussels this difficulty had been overcome by the establishment of serum centres, but in small towns it had apparently sometimes been insuperable and had led to the adoption of other methods. Degkwitz's sheep serum, against the use of which Schlossmann [4] had recently protested "in the interest of the reputation of German science," had generally been proved to be a failure, for it not only did not afford protection but it also caused severe serum sickness. He (Dr. Rolleston) had had some encouraging experience from the use of Tunnicliff's goat serum, kindly supplied him by Dr. O'Brien, but he had not been able to try it on a sufficiently large scale in order to arrive at any definite conclusions as to its value. Another method was active immunization by injection of minute quantities of virulent measles blood diluted in saline, which had recently been introduced by Debré and Joannon,[5] the advantage of this method being that it conferred a permanent immunity without producing the disease as in sero-attenuation. He (Dr. Rolleston) did not think that convalescent serum need be used on so large a scale as Dr. Copeman suggested. As he had remarked at a previous meeting,[6] he was not in favour of its application either in well-to-do families or in hospitals with an hygienic environment, as measles contracted under such conditions usually ran a mild course.

Dr. A. SALUSBURY MACNALTY said that he was glad that Sir George Buchanan had referred to the fact that the Ministry of Health had for a number of years concentrated on the important question stressed by the author of the paper, namely, the great risk of an attack of measles to children under 3 years, or rather under 5 years of age. Dr. Copeman had outlined an attractive scheme in "sero-attenuation" for the protection of children from measles at the earlier ages. He agreed, however, that it would be difficult to obtain donors of serum. He (the speaker) mentioned that when investigating the epidemics of poliomyelitis last year

1 *Thèse de Paris*, 1923, No. 287.
2 *Deutsch. med. Wchnschr.*, 1926, lii, 1241.
3 *Wien. med. Wchnschr.*, 1924, lxxiv, 1027.
4 *Klin. Wchnschr.*, 1926, v, 1824.
5 "La Rougeole, Epidémiologie, Immunologie, Prophylaxie," 1926.
6 *Proc. Roy. Soc. Med.*, 1926-7, xx (Epid. Sect.), 9.

he had endeavoured to arrange for the treatment of patients by convalescent serum, but none of the parents of convalescent cases of poliomyelitis would sanction the taking of serum for this purpose. Dr. Copeman's scheme would not supersede the administrative measures now in force for measles and urged by the Ministry of Health, as regards health-visiting, home-nursing, hospital treatment, the prevention of complications, especially broncho-pneumonia, and the after-care of patients with a view especially to the prevention of deafness, ocular defects and tuberculosis. Dr. J. D. Rolleston's observation that the hospital mortality from measles was slight was of interest. Some years ago the mortality in the French hospitals from this disease was excessive, broncho-pneumonia spreading from case to case.

Fleet-Surgeon W. E. HOME said that in regard to the occurrence of measles in the War, among Scotch Highlanders and American country recruits, an epidemic of measles in a division of the U.S. Army, on the Texan frontier in 1916, was reported by Col. F. L. Munson in the *Military Surgeon* from June to September, 1916. He considered measles not so much an eruptive fever as an epidemic respiratory disease, with eruption appearing when infectivity was at an end. He therefore dealt with the epidemic by ventilating the tents very thoroughly; of 11,382 troops who followed instructions, only 105 were attacked, 1 per cent., while of 2,411 who paid less attention, 339 (15 per cent.) were attacked. Measles might be deadly in patients under 3 years of age, but it did harm also when contracted later. When he (the speaker) was serving on the *Britannia* at Dartmouth in 1889, he noticed that among certain cadets only those who had had measles made no increase in weight, or but minute increase of weight, during their period of instruction there.

We were now being told that we must not abolish tuberculosis or measles lest the nation should lose its acquired immunity; then this method of cure might become the method of choice of the future.

Dr. GRAHAM FORBES said he much hoped that Dr. Copeman's very interesting account of the serum prophylaxis of measles would, at some future date, result in its addition to the existing schemes in force for the control of the disease. The difficulties in the way of its introduction in this country, however, as mentioned by previous speakers, were likely to prove serious obstacles, but it was possible that time and further experience would surmount them. The school medical service in London, ever since its origin some twenty-five years ago, had devoted every possible attention to the best means of controlling the spread of infection in the schools. He wished to correct the impression conveyed by Dr. Copeman that school closure was generally relied upon at the present day as a control measure. Its value, in London at any rate, had long ago been disproved, and it had not been resorted to for the past fifteen years.

As long ago as 1902, at the instigation of Dr. James Kerr, then School Medical Officer for London, Dr. C. J. Thomas, in co-operation with Dr. Sidney Davies, the late Medical Officer of Health for Woolwich, had carried out over a number of years a very thorough investigation into the comparative values of two schemes for the control of infection in the elementary schools of Woolwich. In one half of the borough, on the outbreak of measles in a school, closure of the infants' classrooms was put in force; in the other half, a warning card was issued to parents, and only children unprotected by previous attack were excluded. A complete register was kept of the measles history of each child, from which the necessary information was later compiled.

The result of the inquiry showed that school closure had no material effect in checking the spread of the disease. The percentage of unprotected children attacked varied in different localities of the school-closure area between 13 and 90, and where closure was not in force, but exclusion only of the unprotected, the percentage attacked ranged between 22 and 62. Consequently school closure as a measure of control was definitely given up by the school medical service as long ago as 1909.

Since that date, exclusion of the unprotected in the infants' departments had been to a large extent relied upon, and, as it had been well realized that very few eventually escaped infection, attention had been mainly directed to postponing the attack till after the age of five, to averting serious complications by the provision of adequate nursing attendance in the homes, and so to reducing the heavy mortality in the under-five-year period.

Additional efforts during the last few years had included a further joint scheme of control, by which, as soon as an outbreak began to assume epidemic proportions, utilization was made

of the school nurses for the daily visiting of infants' departments in order to inspect children under five years of age, and to discover and exclude cases in the earliest stage of attack. In addition, the attendance officer was required to look up absentees in order to ascertain whether absence was due to measles, or illness suspicious of its onset. Co-operation with the school medical officer and the borough medical officer ensured the acquisition of early information as to such cases being sent by the head teacher, school nurse or attendance officer. It was also arranged that visits to infected homes should be paid by the health visitor for report as to the need for supply of nursing help through the district association. Facilities for admission to hospital of measles cases had also been recently provided by the Metropolitan Asylums Board, which would be available in readiness for the next epidemic, when it was hoped that the scheme of conjoint effort would effect a still further reduction in the number of fatal cases and obviate serious complications.

It had appeared that, although the measles incidence in the schools had shown no diminution, mortality had been very materially reduced, following on the gradual improvement in, and constant attention to, the control of infection in schools. Thus in the past fifteen years the quinquennial mortality had fallen from over 9,500 to less than 4,000 for all ages.

	Measles, cases in schools	Measles, deaths all ages
1911-1915	116,754	9,596
1916-1920	115,093	5,830
1921-1925	116,956	3,859

Further, in the two latest epidemics of 1923 to 1924 and 1925 to 1926, the number of deaths had declined from 1,391 to 1,034. The age grouping of the 1,034 deaths in the last epidemic showed that only fifty-five occurred in the school age group of over 5 years, and five at over the school age of 14, whereas 248 were under 1 year, 462 between 1 and 2 years, and 264 between 2 and 5 years. Hence the importance, long realized, of directing every attention to the under-five-year age-period as regards prompt recognition, home conditions, nursing attendance and, in certain cases, removal to hospital.

The future might show that serum prophylaxis afforded a means of still further reducing the heavy loss of child-life with the help of the existing organization.

Dr. W. A. LETHEM gave a brief description of the trial of Degkwitz' measles serum in this country; one of the first cases in which it was administered showed severe serum sickness and its use was stopped. Foreign reports tended to show that it gave no protection and that serum sickness was common. He outlined the investigation at present being conducted by the Medical Research Council.

Dr. G. CLARK TROTTER (Medical Officer of Health of Islington) emphasized the value of efficient nursing in measles, which, he believed, would bring down the death-rate of this disease still lower; the value of this nursing, even in the home, could not be over-estimated.

As regards the severity of measles in adults who came from parts of the world where the disease was a rarity, he instanced his experience of what had occurred in a Newfoundland regiment in this country during the war period.

Dr. STELLA CHURCHILL said she had found during an epidemic of measles in Bermondsey that there was great mortality among children under five who were removed from their homes. She had pressed the local authority for a trained nurse specially to look after children in their own homes; she had found this was not always welcomed by the practitioner in charge of the case.

In view of present public opinion on immunization in general she thought the reader of the paper would probably find much difficulty in advocating treatment of measles in the manner suggested.

Dr. T. S. MCINTOSH (Medical Officer of Health, Hendon) said that the subject of Dr. Copeman's paper was particularly interesting just now when methods of prevention by means of active immunity were so much to the fore. If a satisfactory method of immunizing against measles or whooping-cough could be devised there would be a very strong case for its application on a large scale, because the great majority of people contracted these diseases, a fact which placed them in marked contrast to scarlet fever and diphtheria.

90 Copeman: *The Prophylaxis of Measles*

A particularly interesting point about the process which Dr. Copeman called "sero-attenuation" was that (so far as he [the speaker] was aware) it introduced an entirely new method of producing immunity, namely, by allowing the patient to acquire the disease naturally while modifying its severity. That might be described as "controlled natural immunity" rather than "artificial immunity," and it was reasonable to suppose that it might be more complete and lasting than an artificial immunity.

He (the speaker) thought that the difficulties attendant on the application of a process which entailed the use of human serum would probably prevent any very general adoption of the method. But acquaintanceship with the method was most valuable as a means of increasing our knowledge of the disease and, if this method should prove to be useful, it was extremely probable that some means would be devised of substituting some other product to replace the use of human serum.

Section of Laryngology.

President—Dr. ANDREW WYLIE.

Case of Sinusitis.

By T. B. LAYTON, D.S.O., M.S.

A. T.—FIRST seen May, 1925, for headaches—pan-sinusitis on right side.

Treatments.—Hot light bath to head, right antrum washed out several times. Pus returned in middle meatus after antral washed out. Pus high up on right side with post-nasal mirror.

June.—High septum operation to relieve pressure in mid-turbinal region. Suppuration between flaps followed by considerable swelling of mucous membrane.

September, 1925.—Right radical maxillary sinus operation (Denker) under intra-tracheal general anæsthesia (Mr. Gill-Carey).

October, 1925.—Pus in left antrum. Washed out twice a week.

November, 1925.—Severe headache. Pus returned after antrum washed out.

November 17, 1925.—Radical operation on left maxillary sinus, local anæsthesia; Caldwell-Luc.

December 17.—Hole in antrum small, high up and far back. On washing out, no pus, but still complains of headache.

February, 1926.—Pus in middle meatus after washing out antrum.

March, 1926.—Front end left middle turbinal removed, local anæsthesia.

November, 1926.—Pus in left antrum again, left maxillary sinus re-opened, Caldwell-Luc operation converted into Denker. Mucous membrane of antrum appeared normal. Re-admitted one week later for severe head pains.

Condition at present.—No sign of sinus disease, but is not entirely free from pain.

The case is shown: (1) To demonstrate the value of high operation on septum and high removal of middle turbinal for drainage of frontal sinus.

(2) To compare the value of Caldwell-Luc and Denker's operation.

(3) The patient having had one operation under general and four under local anæsthesia, to confute those who say that the advantages of local anæsthesia cannot compensate for the inconvenience of a bad mental disposition and terror on the part of the patient.

Discussion.—Dr. ANDREW WYLIE (President) asked as to the method of anæsthesia.

Mr. LAYTON replied that he always operated on the maxillary sinuses by block anæsthesia combined with infiltration. First he cocainized the septum and lateral wall of the nose with cocaine ointment anæsthesia. For the block anæsthesia he had varied from inside the mouth to the external methods. Externally, he had been using the method which went through the orbit. Originally within the mouth he used the method taught him by Mr. Graham Brown, of Brisbane, in which the needle was put into the mucous membrane at the back of the tuberosity of the maxilla. Either was quite efficient. He adopted the mouth procedure if the

54 Layton—Hutchison—MacLeod

upper back molars had been removed on that side. If the mouth was somewhat small he adopted the external route. There was another method through the mouth, but not so far back, which was efficient in many cases. In this the needle was inserted opposite the fang of the second upper molar. With it, however, one might not catch all the branches going to the back of the antrum, and the portion of the nasal fossa not touched by the block anæsthesia in this method was that below the inferior turbinal, therefore one must also infiltrate this area from the inside, the most difficult injection of all, because of the way in which the inferior meatus turned outwards. He always infiltrated sublabially. One was not bound to block. Sublabial infiltration associated with packing the antrum with 5 per cent. cocaine after it had been opened was a possible means of obtaining anæsthesia, but not so efficient as the block anæsthesia.

Chart showing Striking Result of One Injection of Autogenous Streptococcus Vaccine.

By Arthur J. Hutchison, M.B.

Schoolmaster, aged 44. About the seventh day of influenza, after the temperature had been nearly normal for two days, the nose became blocked, temperature rose and "headache behind the eyes" was complained of. On the thirteenth day the left cheek became red and puffy. On the fourteenth day a bleb appeared on the left cheek, and on the fifteenth day another on the right cheek and bridge of the nose. The right antrum contained some pus, the left contained pus under pressure. I opened both antra through the inferior meatus; thereupon the temperature fell by steps to 100, but rose again on the nineteenth day to 103. Meantime an erysipelatous flush was spreading over the forehead and into the scalp. On the twentieth day an autogenous vaccine (10,000,000 pure streptococcus) was given at 1 p.m. At 10 next morning the temperature had fallen to 97·6° F. and it has never risen above 99° since. The flush over the forehead and scalp disappeared in two days and the discharge from the antra very rapidly ceased.

Mr. J. F. O'Malley said this was a very interesting case, as in it one seemed to see a condition in which, primarily, there was an intranasal infection. Whether the beginning of it was influenzal or streptococcal seemed doubtful, but the subsequent developments were manifestly streptococcal, and the continuance of the condition from the antral infection to the subcutaneous infection raised the question as to whether one was here dealing with a single kind of organism. The facial condition was the usual type of facial erysipelas, and the fact that the autogenous vaccine prepared from the pus from the antrum was so effective in the treatment of the pyrexia, raised the question whether the original infection in the antrum was not due to the *Streptococcus erysipelatosus*. He had not seen a record of primary infection of the antrum being due to *Streptococcus erysipelatosus*.

? Actinomycosis of Tonsil.

By A. L. Macleod, M.B.

Male, aged 33. Seen February 5, 1927. Swelling of left tonsil, two months' duration. No pain. No enlarged glands in neck. Wassermann reaction negative.

February 26.—Piece removed for section.

March 9.—*Report*. Section shows a very vascular inflamed tonsil. It is crowded with organisms. No tubercle bacilli found and there is no evidence

of neoplasm. There are large numbers of a Gram-positive sporothrix present which has the microscopical appearances of actinomycosis. The microscopical appearances of the tonsil are in favour of that disease.

Discussion.—Mr. J. A. GIBB asked what treatment had been adopted in this case.

Dr. A. WYLIE (President) asked whether Dr. Macleod had punctured the swelling. He suggested a free incision into it, to see what were the contents.

Dr. W. H. KELSON said he thought there might be deep-seated pus.

Mr. H. BELL TAWSE said he thought this swelling seemed rather soft for actinomycosis of tonsil, but there might be pus there also. Only a microscopical examination could enable a decision to be made.

Dr. MACLEOD (in reply) said there had been no treatment; he had kept the case three weeks in order to obtain the benefit of Members' opinions. He removed a small piece for sectioning, and he had intended to show three sections from the case, but he came away without them. He would report later.

Pharyngeal Diverticulum.

By HAROLD KISCH, F.R.C.S.

THE patient presented himself at the hospital with typical symptoms of a pharyngeal diverticulum, the presence of which was confirmed by X-ray examination. The sac was removed by a two-stage operation. At the first stage the sac was isolated up to the neck in the usual way. At the second, the sac was removed in the following manner: A purse-string suture of silk was inserted through the pharyngeal muscle around the orifice of the sac; the neck of the sac was then crushed by an appendix clamp, and after a short interval the clamp was removed, the sac cut off, and the stump touched with pure carbolic acid. The stump was then invaginated into the pharynx, and the purse-string suture tied. No other suture was used. The skin incision was closed, a drainage tube being left in position. Intratracheal ether was the anæsthetic employed, and no feeding-tube was used. The patient made an uneventful and uninterrupted recovery.

X-ray photographs of the condition, before and after operation, are shown.

Discussion.—Mr. A. J. M. WRIGHT, commenting on the remark that no feeding tube was used, said he regarded this as a very important point. He thought the surgery of these œsophageal cases was now passing through the same evolution as the surgery of gastric cases. In treatment of the latter kind of case, in former days, the surgeon starved the patient for a long period, fearing leakage at the junction. He felt sure there was less risk if natural feeding was carried out soon after the operation than if a tube was inserted into the œsophagus. The damage done to the patient by starvation was much greater than that which might occur by running the doubtful risk of leakage.

Mr. NORMAN PATTERSON asked what was the interval between the first operation and the second.

Mr. KISCH (in reply) said that if he obtained good closure, he did not use a feeding tube. If he thought a tube was likely to be necessary he used it for a week beforehand in order to accustom the patient to it. He did not use a tube if he could avoid it, and he had not had trouble from the omission. A week elapsed between the two operations.

He showed this case: first, because there was a slight alteration in the technique; secondly, to obtain opinions as to the danger of operating on this condition. During the last

FIG. 1.—Lateral view, before operation.

FIG. 2.—Antero-posterior view, before operation.

FIG. 3.—Lateral view, after operation.

FIG. 4.—Antero-posterior view, after operation.

year he had had several cases in which he had advised operation, and they were referred to well-known Members of this Section, who advised against operation. These patients were persons aged 55 to 60. One such patient was obviously starving, and he (the speaker) proposed to operate upon him. He went to another Member, who said the operation was too dangerous and that the man was too ill. It was not done, and the man died of starvation. He ought to have been operated upon, and he thought the proper thing, in that case, would have been to suspend the pouch and allow the man to take food. After feeding up, removal could be done at a second operation. The surgeon ought not to allow a man with this condition to starve.

The other man whose case he had in mind was in good condition; he had a sac which he could empty himself. He (Mr. Kisch) advised operation because the patient was very uncomfortable and because there was danger in leaving such sacs. In one case a sac was left and the man developed acute empyema in twenty-four hours. At the post-mortem the sac was found to be full of pus, the pleura had been directly infected from the sac. He did not know whether any Member had seen malignant growth arising from such sacs, but such an occurrence was a possibility.

This man was now well, but he still had his sac. He had hoped this matter would have been discussed. The danger alluded to by the surgeons mentioned was that of mediastinitis following operation. Apparently, when this operation was first performed, somebody had a fatal case, which had been reported over and over again; the supposed risk had therefore become exaggerated. He did not see why these cases should not be dealt with surgically. There was, of course, more risk in operating on these cases than in removing an appendix, for in an abdominal operation the peritoneum helped one, whereas here there was no peritoneum. If the exit of the sac were properly exposed, there was no difficulty in closing the orifice, and in his cases he had had no trouble or anxiety in regard to leakage.

Chronic Hypertrophic Laryngitis, treated by Diathermy.

By HAROLD KISCH, F.R.C.S.

MRS. B. presented herself at hospital with a history of many years' hoarseness, and almost complete loss of voice for two years. There was a grossly deflected nasal septum, and the larynx showed a swelling in the interarytænoid region, which occupied the posterior third of the laryngeal orifice. The usual examinations excluded tuberculosis and syphilis. Under an anæsthetic the swelling was burnt away with diathermy. The patient now has a good voice. A submucous resection of the septum will be performed later.

Discussion.—Dr. JOBSON HORNE (quoting the description in the printed notes of the case) said that the words "the patient now has a good voice," constituted a relative term. There was, undoubtedly, still hoarseness, and prolonged talking would probably produce again "almost complete loss of voice." The present condition, compared with that as given in the notes preceding the application of diathermy, did not appear to show any marked improvement. Moreover it was not reasonable to expect the application of diathermy to "a swelling in the interarytænoid region" in a case like the present one to produce "a good voice," for the reason that the interarytænoid swelling was not the main cause of the hoarseness. The case was one of pachydermia laryngis, with thickening of the vocal cords and the ventricular bands. General treatment would be of more value than local treatment; depletion, if not bleeding, was required.

Mr. T. B. LAYTON said he was glad to see Mr. Kisch trying a new line of treatment of pachydermia. He was doing what Dr. Jobson Horne suggested, but was getting the depletion from inside the larynx, rather than from some point outside. He was sorry Dr. Horne threw cold water on this attempt, as he (the speaker) had been most disappointed with the results in such cases. Admittedly it was the nasal obstruction which was largely the cause of the laryngitis, but his experience had been that submucous resection did not do the larynx

any good. Probably it had to be done in this case in order to give the diathermy a better chance. Having secured a relative improvement in the voice in this patient, it might be well to try to get rid of the swelling somewhat by further diathermy. He asked what anæsthetic Mr. Kisch used and how it was administered. He would like to hear Dr. Jobson Horne's evidence for the statement that it was not the swelling in the interarytænoid region which was the cause of loss of voice. And if it was not, what was the cause in such cases?

Dr. JOBSON HORNE, answering questions put by Mr. Layton, said that swellings or excrescences in the interarytænoid space, when seen clinically, suggested that their removal would produce an improvement in voice. That suggestion was misleading. For when seen post mortem they were found to be so far above the level of the vocal cords and the level of the ventricular bands that it was obvious their presence was not the material cause of the hoarseness or of the loss of voice. That was due to a thickening of the vocal cords and of the ventricular bands, which was invariably present in cases of pachydermia laryngis presenting interarytænoid excrescences. He (Dr. Horne) hoped to have an early opportunity of demonstrating and describing more fully the points he had touched upon. By bleeding he meant, of course, phlebotomy, not local bleeding. Blood-letting and rest of voice would be helpful, but nothing would be gained by any electrical burning of the larynx.

Mr. HERBERT TILLEY said that in this patient's case there still remained some thickening, and Mr. Kisch might be tempted to make a second application of diathermy. He asked whether œdema of the larynx or difficulty of breathing followed the procedure.

He thought there was much in what Dr. Jobson Horne said as to the pachydermia being a local manifestation of a general condition of fibrosis. In the University College Museum there was a specimen of a large interarytænoid pachydermia, which occurred in his (the speaker's) clinic many years ago. That patient suffered from hoarseness and difficulty in breathing. Dr. J. W. Bond saw the case with him and considered it malignant. A week or two after treatment the patient had died, it was said, "after a fit of choking." He managed to secure the larynx, and also a small fragment of the liver, which had all the characters of gin-drinker's or hobnailed liver. Therefore it was an instance of more or less generalized fibrosis of alcoholic origin. He did not know how far alcoholism might be a factor in Mr. Kisch's patient. It was difficult to get rid of the localized laryngeal hyperplasia, and possibly for the reason that the disease was a purely local one.

Dr. W. H. KELSON said that Mr. Kisch had obtained a brilliant result in this case. It was very important that the case should again be brought before the Section in six months' time to see whether the improvement seemed to be permanent.

Dr. NEW (U.S.A.) said that a few months ago he saw a case which rather emphasized the local aspect of such a condition. It was that of a man who, for eight years, had been working in a room in which there were always steaming towels. He developed typical pachydermia, and later had to submit to tracheotomy. Later still, on the recommendation of Dr. Lewis, of Philadelphia, and Professor Chevalier Jackson, laryngectomy was carried out. In that man's case there seemed to be no general reflex of the local disease.

Mr. KISCH (in reply) said the patient's voice was now 75 per cent. better than before: her larynx had been half filled with the protuberant mass, and she had had to resort to writing down her wishes. He left it to Members to decide whether interference with talking was due to the swelling in the interarytænoid region.

Chloroform was the anæsthetic; a directoscope and an ordinary sized electrode were employed. The difficulty was in deciding how deep to go, because of the risk of causing fixation of the crico-arytænoid joint. On the whole, he was fairly satisfied with the result. The question of applying the diathermy further was a very serious one. Though the patient could not sing in the choir, she was fairly satisfied with her voice, and if he did more he might render her condition worse functionally. He had removed similar growths with the knife, and they had recurred quickly; he regarded the result obtained by this diathermic method as better than by any of the other means he had tried. He would bring the case forward again next year. Submucous resection would, he thought, make the patient much more comfortable. He wondered, in view of Dr. Jobson Horne's criticism, what he (Dr. Horne) would have done if the case had come before him. There was no trouble from œdema.

60 Jobson: *Recurrent Paralysis;* Gibb: *Tumour Involving Orbit*

Double Recurrent Paralysis caused by Tuberculous Glands.

By T. B. JOBSON, M.D.

MISS I. T., aged 37. Admitted to City of London Mental Hospital, June 16, 1922, with chronic delusional insanity.

Examined on admission. Nothing abnormal in chest. Weight 10 st. 9½ lb.

Extracts from Case Sheets.—September 29, 1926.—"Has lost two stone since admission." "In rather poor health." "Complains of sore throat, but nothing can be seen, or felt." "*Nil* in heart and lungs." "X-ray did not show anything abnormal."

October 27, 1926.—"Still complains of her throat."

December 12, 1926.—"Voice husky, stridor at times." "Coughed up a little blood."

December 21, 1926.—Examination by Dr. Jobson. Larynx showed double recurrent paralysis, more marked on right. Adductors involved as well as abductors.

X-rayed again. Skiagram suggested tuberculosis.

Wassermann reaction negative.

January 26, 1927.—"Slight right external strabismus noticed." "Complained of pain in eye and right ear."

February 17, 1927.—"Difficulty in swallowing increasing." Able to take very little food. Breathing stridulous. Temperature 100° F. to 101° F.

February 21, 1927.—Died.

Dr. Robinson, to whom I am indebted for this specimen, considers that death was hastened by the pressure on the vagi. Post-mortem specimen shows tuberculous glands around root of the lung.

Discussion.—Dr. ANDREW WYLIE (President) asked whether Members had seen many cases of double recurrent paralysis of the larynx caused by tuberculous glands.

Mr. J. A. GIBB said he had had a case due to extensive tuberculous glands on the right side of the neck. Directly the discharge had ceased and healing had taken place the patient became affected with paralysis of the right vocal cord. This had happened two years ago, and the question was whether operation would do any good; he thought not. During the War he carried out scar-tissue operations on the arm, etc., and some of the cases did well.

Tumour Involving the Orbit and certain of the Nasal Sinuses.

By J. A. GIBB, M.D.

M. B., AGED 61.—Referred to me by Eye Department on September 11, 1926.

Examination.—Left eyeball proptosed and some swelling at inner part of orbit, pushing eyeball outwards and downwards. It was thought to be a mucocele of the frontal sinus.

Operation, September 19, 1926.—Upon incision it was seen to be growth involving the soft tissues of the orbit. As no permission had been obtained to excise the eyeball the wound was sewn up after a piece of the tumour had been excised for examination. The next day she was informed of the seriousness of her condition. She declined any further operation.

November 11, 1926.—Presented herself at the hospital, having made up her mind to undergo any operation necessary. The growth was seen to have increased.

Ophthalmic report: Vision, right $\frac{6}{12}$; disc, right, normal; vision, left, $\frac{6}{60}$; nerve head shows signs of pressure; no atrophy, disc blurred; eye tender to pressure.

Operation.—Planned so as to expose growth and to completely remove it, this was found to involve tissues of orbit, ethmoid, frontal sinus, and to be prolonged into, but not growing from, the maxillary antrum. The orbit was completely exenterated and the dura mater of the middle fossa exposed. After careful suturing, the cavity was firmly plugged with gauze soaked in acriflavine $\frac{1}{500}$.

Pathological Report: (1) Type similar to parotid tumour. Slender columns of endothelial cells lying in an abundant hyalomyxoid base. (2) Endothelioma with a good deal of fibrosis round tumour masses.

Discussion.—Mr. W. M. MOLLISON said he feared there was recurrence in the region of the nasal process of the maxilla, perhaps involving the antrum. He suggested that when Mr. Gibb operated on the patient again he should apply diathermy to the walls of the cavity at the end of the operation. He (the speaker) had done that on two or three occasions and thought the result better. The patient's teeth were in a very bad state and they should have all been removed before operation. [Mr. LAYTON: What about applying diathermy to the outer side of the dura mater?] No, not to the dura mater.

Mr. J. A. GIBB (in reply) said that the sulcus had filled up extraordinarily since the operation. The nasal process of the maxilla was removed completely, and the antrum exposed. The growth projected into the antrum; it was not attached to the antrum. The nasal bones, the lachrymal bone, the os planum and lower part of the wing of the sphenoid were taken off, and the floor of the orbital cavity, the floor of the frontal sinus, and the middle fossa were exposed. He plugged the cavity firmly with gauze steeped in 1 in 500 acriflavine. Breathing and pulse almost ceased. The house surgeon sent for him. He removed the plug which was causing indirect pressure on the medulla; breathing and pulse quickly resumed.

In a case of carcinoma which he had previously shown, there was an excess of lipoid in the cancer cells, and there was a great excess of lecithin-lipoid in all new growths. In 6 gr. weight of granuloma removed he obtained ½ gr. of pure lecithin-lipoid. In a wart or a fibroma there was more lecithin-lipoid in the cells than in carcinoma. This seemed to show that there was increased irritability of cells in people who had new growths, and there was likely to be a greater growth of cells as a result of irritation than in the case of cells in which there was only the normal trace of lecithin-lipoid. A point of interest would be as to whether cholesterol, which was anti-lecithin-lipoid, was or was not found in carcinoma. If it were found in carcinoma, one would say that the patient had a better chance of recovery than if cholesterol were not present. Did Mr. Mollison think that deep X-rays would have an influence in preventing recurrence in this case? A further operation on this patient would not be welcomed.

Mr. MOLLISON (in reply to Mr. Gibb) said he had great faith in the application of deep X-rays in the treatment of these growths.

Tracheotomy Tube removed from a Woman aged 58, after being present for Eighteen Months.

By NORMAN PATTERSON, F.R.C.S.

THERE was a small fistula in the neck and the tube was completely buried amidst granulations; the detached portion of the outer tube was difficult to find. A fresh tube was inserted and kept in position for fourteen days. The condition for which the original tracheotomy was performed was not ascertained.

62 Macleod—Negus—Jewell

Case for Diagnosis.

By A. L. MACLEOD, M.B.

G. B., AGED 52, male. Clerk. Loss of voice six weeks. No dysphagia. No cough. No expectoration. Not losing weight. Marked swelling of ventricular bands and arytænoids. No ulceration seen. Wassermann reaction negative.

Discussion.—Dr. JOBSON HORNE said he was inclined to regard the condition as due to tuberculosis. In addition to the loss of voice for six weeks, he understood that nine months ago the man had lost his voice for a while. On the glottic surface of the left arytænoid there was some ulceration and at a site which was very suggestive of tuberculosis. He would anæsthetize the larynx with cocaine and adrenalin, using a suitable laryngeal syringe so that the solution trickled over the epiglottis and found its way along the ventricular bands and arytænoids, inducing some anæmia. Then one could form a better idea of the starting-point of the disease. While the larynx was under the local anæsthesia he would remove a piece from the glottic aspect of the left arytænoid for microscopical examination.

Mr. J. F. O'MALLEY said that this lesion looked suspiciously like tuberculosis, but it might be early carcinoma, for now one could see, in addition to the swelling of the arytænoid, a slight mass of new tissue on its anterior and also on its outer aspect. In between, there was a moist depression suggesting an ulcerating area. If it was early carcinoma, it was extrinsic and it would soon prove itself by secondary infection of the glands. At present he could not find any enlarged glands. He agreed with Dr. Jobson Horne that a portion should be removed of what appeared to be increase of tissue from the arytænoid; probably that would give an indication as to the nature of the lesion.

Dr. MACLEOD (in reply) said that the suggestions made would be carried out. When the notes had been prepared, a fortnight ago, there was no sign of ulceration.

Fibroma of the Pharynx, with Malignant Changes at its Base.

By V. E. NEGUS, F.R.C.S.

THIS growth apparently started as a simple fibroma. The patient was seen in October, 1926, when the pharynx was almost completely filled, and there was considerable dyspnœa. After removal of the tumour carcinomatous changes were seen in sections. Lateral pharyngotomy and removal of glands have been done; the glands were innocent. It is too early to decide as to the prospect of cure; the case is shown because of the interest attached to the malignant changes in what was probably a simple tumour.

Ulceration of Vocal Cord; ? Malignant.

By W. H. JEWELL, M.D.

F. M., AGED 48. This patient was shown at the last meeting under the above heading.[1] There was a division of opinion as to the diagnosis. The majority advised against removal of the cord as they considered it was tuberculous. Temperature chart shows no rise of temperature and skiagram of chest shows no abnormality. The temperature was recorded for a month, and we have had a

1 *Proceedings*, 1927, xx (Sect. Laryng.), 46.

skiagram taken; and no active mischief is apparent. There were a few calcareous glands. The apices were normal. Two physicians have seen the case, and cannot detect anything in the lungs. I think the condition is malignant, and that I am justified in attempting to remove that cord. There could not be much harm done, and it will probably relieve his pain. It ulcerated through in three weeks, and there has been no constitutional disturbance, as there would have been, I think, had it been tuberculosis.

Discussion.—Mr. NORMAN PATTERSON said he thought the ulceration was carcinoma, but the opposite cord seemed to be involved. As there was no sputum he would take a direct swab from the larynx and inject the material into a guinea-pig to see whether tuberculosis could be excluded. Laryngectomy was the only treatment for a growth of this extent.

Dr. JOBSON HORNE said he did not think that, clinically, the case was suggestive of tuberculosis; the ulceration was confined to the vocal cord, and if the disease were due to tuberculosis the ventricular band and other parts of the larynx would also be involved.

Dr. ANDREW WYLIE (President) said he thought the case was malignant, and that the fellow cord was affected too. Total laryngectomy seemed indicated.

Mr. J. F. O'MALLEY said that though this would most likely prove to be carcinoma, occasionally one saw strange cases of tuberculosis with deep excavations of the kind this case presented. And the fact that there was a second lesion did not entirely exclude that possibility.

In one striking case he had had there was a good deal of controversy with the War Office, the patient being an officer whom the speaker saw during his service at the Herbert Hospital at Woolwich. Clinically, it was like this case, and his diagnosis was tuberculosis of the larynx. No tuberculosis could be found elsewhere, and syphilis was excluded. The patient was under observation for some time. Six months later he died from acute tuberculosis. He did no think this present case was tuberculous.

Mr. JEWELL (in reply) said that the left cord was completely fixed; there was free movement of the right one. He (the speaker) thought he would remove the cord.

Postscript: April 22, 1927.—A portion has been removed from the left cord, and the pathologist reported it to be an epithelioma. A few days later laryngo-fissure was performed and the left cord was removed, together with the surrounding tissues—the cricoid being divided in order to remove them more freely in the subglottic region. A portion of the right cord was also removed.

Growth of Palate.

(Shown at the last meeting of the Section) By H. BELL TAWSE, F.R.C.S.

E. A., FEMALE, aged 35.

Pathological Report.—"The section shows a basal-celled carcinoma of relatively innocent type, homologous with the so-called mixed tumours of the salivary glands. In this case it is not difficult to trace its origin to the ducts of mucous glands."

Microscopic slide shown.

In view of this report suggestions are asked for as to the extent of operative treatment indicated.

Discussion.—Mr. TILLEY said that complete removal would involve a very extensive operation and might be productive of no more benefit than if radium needles were inserted. The present method was to insert four or five needles with one milligramme of radium

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in each, leaving them *in situ* for four days. Some Members had seen this type of treatment in the Radium Institute, Brussels, and with extraordinarily good results; he referred to cases of epithelioma of tonsils and neighbouring regions. If radium did not do what might be expected of it in this case a wide removal could subsequently be carried out, and then the specimen Dr. Horne had asked for could be provided. To remove a portion of the growth now for section might involve a certain degree of septic infection.

Mr. C. A. S. RIDOUT said he would remove a large piece, and carry out a wide destruction by application of diathermy.

Dr. JOBSON HORNE said there seemed to be some doubt about the malignant nature of the growth. If, as stated in the report, the growth were "a carcinoma of relatively innocent type, homologous with the so-called mixed tumours of the salivary glands," then its removal by surgical interference was liable to be followed by a recurrence and metastases of a less innocent type. For that reason he was in favour of treatment by diathermy.

[*May* 6, 1927.

An Epidiascopic Demonstration of Chronic Hypertrophic Laryngitis, with Some Observations on the Treatment.

By W. JOBSON HORNE, M.D.

Dr. JOBSON HORNE said the subject was large and the time allowed was ten minutes. It would not be possible, therefore, to do more than touch upon some of the outstanding clinical features and to summarize the results of his own investigations. He would speak about the excrescences met with in the interarytænoid space and at the site of the vocal processes in that form of chronic hypertrophic laryngitis commonly called pachydermia laryngis verrucosa.

In the first place he must be allowed to stress the fact that the laryngoscope, after revealing important clinical signals and the wonders of the larynx, was in a sense a superficial instrument inasmuch as it showed the larynx only in one plane, and it might be misleading unless laryngoscopy were followed up by visits to the post-mortem room or to the museum to ascertain the unseen extent of the disease and the relationship of the parts above to the parts below.

INTERARYTÆNOID EXCRESCENCES.

The foregoing remarks were particularly applicable to interarytænoid excrescences or growths as they were called. When such a growth was of unusual size, clinically, the laryngoscope suggested it was in the same plane as the vocal cords, interfering with the vocal function, causing the hoarseness or the loss of voice, and calling for removal. As an anatomical and pathological fact that was not so.

By way of illustrating the point, photographs taken during life of the picture in the mirror and post mortem of the opened larynx, were shown. The latter demonstrated the true relationship of the growth to the vocal cords, the growth being in a much higher plane than the cords.

The two photographs reproduced are of a larynx obtained some thirty years ago. It will be observed (Dr. Horne said) that contrary to the usual practice, he had opened it by a vertical incision through the anterior commissure, a method to be commended when, as frequently was the case—as in the present one, the posterior half of the larynx was the seat of the disease.

The larynx presents the characteristic features of pachydermia laryngis simplex verrucosa et diffusa.

In fig. 1 the specimen had been photographed from above. The photograph shows the interarytænoid excrescence above the plane of the vocal cords, also the central furrow giving the excrescence a bilobed structure.

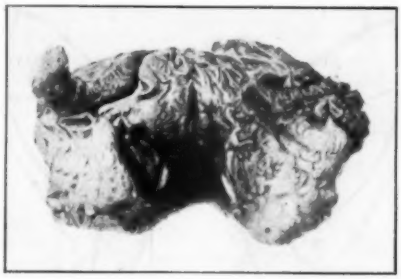

FIG. 1.

FIG. 2.

Fig. 2 shows not only the bilobed interarytænoid excrescence, but also the excrescences and depressions on the posterior parts of the vocal cords, as well as the diffuse nature of the pachydermia.

THE DIFFERENTIAL DIAGNOSIS OF INTERARYTÆNOID EXCRESCENCES.

In 1905, he (Dr. Horne) demonstrated to the Laryngological Society of London [1] a point in the differential diagnosis of interarytænoid excrescences.

[1] *Vide Proc. Laryng. Soc. Lond.*, November 3, 1905, xiii, 12.

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In pachydermia simplex the excrescence is placed centrally and is bilobed. Those two points serve to distinguish it from the tuberculous nodules or granulations so frequent in the interarytænoid space; they are placed more to one or the other side of

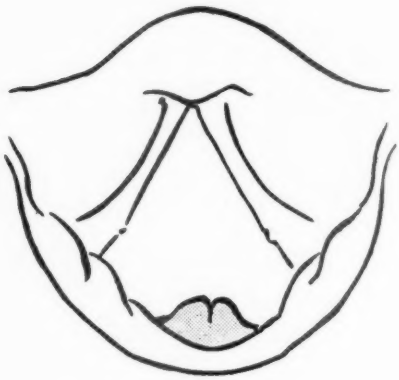

FIG. 3.

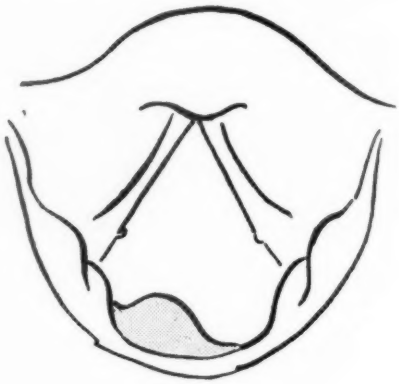

FIG. 4.

FIG. 3.—Diagram of a larynx to show that the interarytænoid excrescence in pachydermia laryngis verrucosa simplex is placed centrally and is bilobed.

FIG. 4.—Diagram of a larynx to show that the interarytænoid excrescence in tuberculosis is placed more to one or other side of the middle line and is not bilobed.

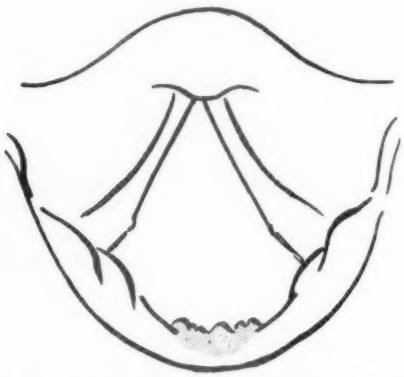

FIG. 5.—Diagram of a larynx to show the nodules or granulations in the interarytænoid space so frequent in tuberculosis.

the middle line, are less uniform, and the epithelial covering is crenated, thinned, and, perhaps, ulcerated. The bilobing is accounted for by the excrescence in pachydermia simplex being merely a hyperplasia of pre-existing tissue, and so retaining the central fissure commonly present in the interarytænoid space.

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Excrescences at the Site of the Vocal Processes.

At one time it was taught[1] that the excrescence at the site of the vocal process on one vocal cord produced by pressure the crateriform depression at the site of the vocal process on the other vocal cord. From the appearances in the mirror that seemed to be a natural explanation. At the same time it did not harmonize with the remarkable absence of complete loss of voice which one would have expected to result from the pressure required to bring about the condition met with. The appearances in the mirror seemed to him (Dr. Horne) to be misleading.

The following is a summary of the results of his investigations communicated to the British Medical Association at Portsmouth in 1899.

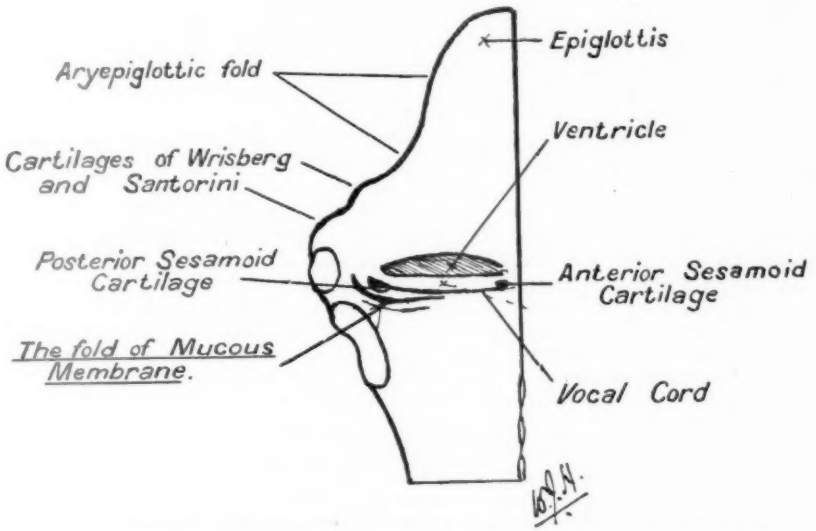

Fig. 6.—Diagram of the interior of the left half of a normal larynx to show the line described as formed by a fold of mucous membrane starting from behind the vocal process of the vocal cord and passing downwards and forwards parallel to and below the vocal cord.

First, in all the specimens examined the depression or pouch was found to be *bilateral*: it was usually more marked on one side than the other, but it was present on both cords.

Secondly, in no case did the pouch or depression, when examined under the microscope in serial sections, present evidence of pressure or attrition. On the contrary, a greater degree of hyperplasia was found. The theory of the formation of the depression by pressure was therefore entirely refuted by histological evidence.

Thirdly, there was histological evidence to show that at certain spots the mucous membrane below the vocal process was more intimately adherent to the underlying cartilage.

[1] "A System of Medicine," by Professor Clifford Allbutt, 1897, iv, 790.

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Based upon his pathological findings, the following is the explanation of the whole question offered by Dr. Horne:—

(1) In the *normal* larynx a line formed by a fold of mucous membrane starts from behind the vocal process of each cord, and takes a crescentic course, passing downwards and forwards, running immediately below the process and parallel to the middle third of the cord. The line is most marked at the vocal process and then thins off. It is more apparent in the male sex and pachydermia laryngis is more frequently met with in the male sex. The mucous membrane immediately above and below this fold is more intimately adherent; hence the fold. The fold itself may be duplicated, and occasionally may be so marked as to suggest, in the image, a second cord.

(2) In pachydermia laryngis the changes in the region of the vocal process—as observed at an autopsy—are symmetrical and bilateral.

(3) When, in the later stage of pachydermia, a warty growth appears in the region of the vocal processes it is not strictly a neoplasm. It is only a localized hyperplasia, or exaggeration of a pre-existent structure, viz., the fold or line of mucous membrane already referred to.

(4) The mucous membrane immediately above and below the part of the fold that has become exaggerated is more intimately adherent, hence the furrow or depression. In this way there is formed at least one depression or pouch on each cord. It follows that the unilateral theory of the pouch cannot stand.

(5) These furrows and excrescences about the vocal processes, being developed at symmetrical spots and in the same plane, presumably would interfere with the apposition of the cords and the production of voice; but, clinically, this apposition is not interfered with—at least, not sufficiently to prevent phonation. The solution of this difficulty rests in the following observation.

(6) By the time the warty condition about the vocal processes was established, *the cords* were found *to be no longer on the same plane.* An alteration in the plane of the cords may be observed clinically, and, *however slight*, at once explains the preservation of the voice. For one cord has to be only on a level very slightly higher than its fellow to permit of a dovetailing, or interdigitating, of the excrescences and depressions and the preservation of the voice.

The alteration in the plane of the cords was verified in the following way: An entire larynx showing typical pachydermia was hardened without being cut open so that the cords were preserved in the same position as at the time of death. The larynx was then set up in hard paraffin and vertical sections were cut at right angles to the vocal processes and the relative positions occupied by the cords were demonstrated as described above.

Some Observations on Treatment.

One point that must first be considered is: Do these cases of pachydermia ever become malignant? He (Dr. Horne) had no hesitation in answering the question in the negative.

Operative treatment in the interarytænoid space had its limitations. Beneath the excrescence is the transverse muscle. Cicatricial contraction may cause laryngeal stenosis. For that reason he (Dr. Horne) was opposed to the application of diathermy in the interarytænoid space. When an interarytænoid growth had to be removed he used fine punch forceps.

Pachydermia verrucosa was always accompanied by pachydermia diffusa, involving the voice-producing parts of the larynx. Moreover, pharyngitis commonly co-existed. It followed from the demonstration that the removal of an interarytænoid growth to

cure the hoarseness would be a fallacy. Pachydermia laryngis is part of a constitutional condition. It is more important to treat the patient than it is to treat the larynx.

Discussion.—Mr. MUSGRAVE WOODMAN asked what proportion of these cases were tuberculous? What were the indications for removal of a portion of the growth by punch forceps. He gathered that Dr. Horne considered removal did not improve the voice, and that there was some risk in removing mucous membrane and leaving bare the deeper surface.

Mr. J. F. O'MALLEY said that with regard to the simpler case, Dr. Horne had made an illuminating remark which probably supplied the key to the question as to the cause, namely, that in these cases there was always pharyngitis, and all the pharyngeal tissues, especially in the uvula, were hypertrophied. It was only necessary to transfer to the uvula the changes seen in the larynx in order to account for the largyngeal condition. The reason for the changes was that the interarytænoid tissues were bearing the brunt of any disturbance caused by the respiratory changes in the mucous membranes of mouth breathers, as seen so commonly in the pharynx. He had long considered that they were entirely due to an attempt by the pharynx and larynx to perform the function which should normally be performed by the nose.

Sir JAMES DUNDAS-GRANT said that he had frequently confirmed Dr. Jobson Horne's generalizations, particularly the statement that when the interarytænoid swelling was either asymmetrical or more on one side than on the other, the condition was almost invariably tuberculous. When the swelling was mesial and somewhat diffuse, it was often due to the influence of secretions from the naso-pharynx and the nose. In a doubtful case one had to exclude the nose as a factor mainly by means of nasal douching, as secondary laryngitis could simulate tuberculosis to an extraordinary degree.

Weleminski had recently carried out investigations as to the effect of large masses of pachydermia of the vocal process on the muscles of the larynx. The pachydermia interfered with the approximation of the vocal processes, and the direction of pull of the vocal muscle and the lateral crico-arytænoids, with disturbance of voice.

With regard to the removal of interarytænoid growths, he had seen these as low as the level of the vocal cords, and so pedunculated that one was able to remove the swelling and so greatly to improve the voice.

The use of the cautery in the interarytænoid space called for great care, as the least wished for result was to produce a cicatrix and adhesions between the arytænoid cartilage. He himself used the cautery in the larynx, but was very careful about introducing it into the interarytænoid region. He did not apprehend any danger in the removal of tuberculous tissue; he had removed a tuberculous growth from the anterior commissure. In another case a bar of tuberculous tissue between the arytænoid cartilages interfered with their function, and kept them apart, so that regurgitation of liquid into the larynx occurred when the patient drank. After removal of the growth the glottis closed sufficiently to enable the patient to drink.

Dr. P. WATSON-WILLIAMS asked whether Dr. Jobson Horne had himself made investigations, or knew of any made by other observers, on the possible association of infecting organisms with these epithelial thickenings; this was suggested by the frequent chronic pharyngitis and nasal conditions. The analogue of these chronic thickenings was seen in the common corn, which was now recognized as a local infection. Was there in these cases some chronic infection which gained entry into the deeper layer of the epithelium and caused the persisting hypertrophy?

Mr. HAROLD BARWELL said that the removal of tuberculous infiltration beneath intact epithelium appeared to do no harm. In the case described by Dr. Horne there was further tuberculous infiltration in the larynx on the epiglottis and elsewhere, and he did not think he would have removed the outgrowth under those circumstances. Still, the second slide exhibited showed that the condition of the larynx considerably improved. He (the speaker) used to employ punch forceps in cases with dysphagia and infiltration of the epiglottis and arytænoids, and the procedure largely increased the patients' comfort. He had seen two

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cases in which the lesion had been confined to the epiglottis, and removal of the epiglottis had resulted in complete arrest of the disease.

Mr. T. B. LAYTON asked whether it was ever of any value to remove a piece from the larynx for diagnostic purposes. There had been many convinced by the arguments against doing so in order to determine whether a given condition was malignant or not. To-day it had been suggested that it was not advisable to remove a piece when the disease might be tuberculosis or a gumma. Were there any cases left in which help would be gained by the procedure?

Concerning the point raised by Mr. O'Malley, he (the speaker) would carry it further. These patients not only had hypertrophy of the structure of the pharynx, but most of them had chronic hypertrophy of subcutaneous tissues generally; the subjects were generally stout people with a tendency to two or more chins, and he had therefore wondered whether there was some diathesis which accounted for the condition.

Mr. A. SHARP (Leeds) said the condition often supervened in long-drawn-out sinus affections. He believed that the easiest and best treatment was electro-cautery. He had used this frequently for tuberculous lesions, especially for interarytænoid infiltrations, and had never known any detrimental cicatrix, or any serious reaction ensue.

Dr. JOBSON HORNE (in reply) said that the proportion of cases of pachydermia laryngis verrucosa due to tuberculosis was small, and the indications for removal of an inter-arytænoid growth in pachydermia verrucosa were few. He (Dr. Horne) did not know of any evidence to support the suggestion of infecting organisms being associated with the epithelial thickenings. He (Dr. Horne) was opposed to the removal with punch forceps of tuberculous infiltration beneath intact epithelium.[1]

Whether it was ever of any value to remove a piece from the larynx for diagnostic purposes, depended upon the piece that was removed and the angle at which the section was cut for microscopic examination.

Dr. Horne was not opposed to the use of the electro-cautery, but he was opposed to diathermy in the larynx.

Specimen of Ethmoid showing Healed Fracture.

By W. M. MOLLISON, M.Ch.

SPECIMEN of ethmoid showing old healed fracture with depression, into which dura mater herniated into roof of nose; also recent damage at operation. From a man, aged 35.

Severe accident fifteen years ago, no taste or smell since, and nasal obstruction. Some months ago polypi removed by his doctor; recently, further operation under chloroform; operator feared forceps had removed piece of brain (this confirmed by microscopical examination). Patient admitted to hospital and died within a week.

Specimen shown to illustrate the dangers of operation on the ethmoid in a case of old fracture of the anterior fossa of the skull.

Dr. P. WATSON-WILLIAMS said that it was fortunate in this case that an expert like Mr. Mollison was able to prove that co-existing conditions exculpated the operator. Apparently there had been absorption of bone in the anterior part of the left side of the

1 "The Treatment of Laryngeal Tuberculosis," by W. Jobson Horne, *British Medical Journal*, November 4, 1905, p. 1188; introduction to a Discussion at the Annual Meeting of the Association, at Leicester.

crista galli, which suggested that the first trauma had been followed by bone absorption which might have affected the region of the cribriform plate. Had the polypoid degeneration extended so high as to involve the cribriform plate and upper ethmoid cells? He himself often felt diffident about trying to obtain a radical cure in cases of chronic ethmoiditis with chronic polypoid degeneration extending very high up, because, however successful one was in avoiding injury to the cribriform plate, one could not be certain there would not be infection spreading up through lymphatics or veins and resulting in meningitis.

Mr. SOMERVILLE HASTINGS said that, with this specimen before him, he found it difficult to feel sure that the surgeon "went through" in the neighbourhood of the injury.

Mr. W. J. HARRISON (Newcastle) said he had recently had a catastrophe of much the same kind when operating on a pensioner who had had two previous operations, one on the frontal sinuses, and, later, another on the ethmoid cells. A great deal of pus had been present. The frontal sinus operation had been performed during the War, and he (the speaker) did not know the extent of the previous operation in the ethmoid region. A skiagram did not give any help; with the mirror some prominence could be detected in the region of the upper anterior cells and there was a good deal of pus always present there. There was a good deal of bronchitis. As the usual landmarks had gone, he had to proceed carefully. While he was using the punch forceps there was a gush of pus, and when he tested with the probe he found he was up against the brain, although he had not removed any bone. The patient developed symptoms of meningitis, and died in forty-eight hours.

Dr. W. H. KELSON said the position of the head during operation was very important. In two unfortunate cases the head had been horizontal.

Dr. F. SPICER said that many years ago he had shown such a case and the patient had been in the sitting posture during the operation.

Mr. MOLLISON (in reply) said that no doubt the old fracture had produced the hernia; there was a depressed scar at the front on the left side and a resulting thinning of the whole cribriform plate. The thinning of the bone accounted for the ease with which the forceps went through. He believed the operator went through behind the hernia.

In the present case the patient had been lying down during operation and the anæsthetic used was chloroform.

Case of Hysterical Spastic Aphonia.

By Sir JAMES DUNDAS-GRANT, M.D., F.R.C.S.

A MIDDLE-AGED woman with five children, subsequently subjected to the operation of hysterectomy, suddenly lost her voice in November, 1926. She had had some domestic anxiety and had been obliged to use her voice excessively. She was first seen by me on February 8, 1927, and was then voiceless except for a soft whisper. Laryngoscopic examination showed such extreme compression of the vocal cords that apparently no vocal vibration could be effected, while for inspiration the cords separated to the fullest degree. There were no signs of tuberculosis. The usual persuasive methods failed to produce any effect; the larynx was sprayed with an anæsthetic solution from time to time, and faradism was employed while a combination of bromides and valerian was prescribed. Her voice suddenly returned in the night a fortnight after the institution of treatment and three months after the first loss of voice.

The case was obviously one of hysteria and not to be confounded with the singular cramping of vocal muscles in preachers which Morell Mackenzie and Semon have described (Heymann's "Handbuch"), and which seems to be resistant to every kind of treatment. I have seen only two cases of this condition.

Peritonsillar Forceps.

Shown by ANDREW WYLIE, M.D.

THE exhibitor has used these forceps for two years. No knife is required; the forceps are easy to manipulate when the mouth is almost clenched and they are easily opened. The handle is constructed so that the operator's hand does not obstruct the view of the throat.

Acute Frontal Sinusitis, with Abscess of Forehead.

By HAROLD KISCH, F.R.C.S.

MRS. S. was admitted to hospital with an abscess in the mid-line of the forehead, just above the root of the nose. There was tenderness over the left frontal sinus, and pus could be seen under the left middle turbinal. An incision was made along the supra-orbital margin, and extended down the side of the nose. The soft parts, including the contents of the orbit, were retracted. The ascending process of the superior maxilla was resected, and the floor of the sinus removed. The sinus was found filled with pus, and there was a perforation the size of a sixpence in the anterior wall. Through this the abscess in the forehead was evacuated. The anterior frontal spine was chiselled away, and some anterior ethmoidal cells were removed. Drainage was provided by a tube down into the nose, and another one passing out through the supra-orbital incision. Convalescence was uninterrupted, the patient is now quite well, and there is no deformity of any kind.

Discussion.—Mr. T. B. LAYTON congratulated Mr. Kisch on the way in which he had drained the subcutaneous abscess. A case had been shown before the Section in which an addition to such an incision had been made a transverse one, leaving an ugly scar. He himself had shown a case in which, to avoid this ugly scar, he had made a flap with the incision up in the hair-line, thence across to the eyebrow, bringing it vertically in the mid-line. He would not do that again, as the flap contracted to a quarter of its former size, and a great deal of tugging was necessary to get it into place. The corners sloughed. It was important to note that in Mr. Hirsch's case drainage of the subcutaneous abscess was effected by way of the frontal sinus.

Mr. F. B. GILHESPY said that in these cases the posterior wall of the frontal sinus should be examined, as a dural abscess or an extradural abscess might be present. There was a risk, however, as this examination was not always possible in this method of operating.

Mr. A. D. SHARP said that two similar cases had been sent to him by the same medical man within the last three months, each with a history of influenza. One patient complained of a swelling in the forehead extending to the hair-line; otherwise he felt in good health.

There was no doubt about the frontal abscess, there was a septic condition of both ethmoidal regions, and polypi were present. An incision was made in the mid-line, and a large quantity of pus escaped. Drainage continued for two days. Some dilatation of the left pupil developed, and the patient became drowsy. The frontal sinuses were laid open, and the anterior wall was cleared; there was much swelling of tissues. Pus welled through from the dura, and its flow increased when the dura was pressed away. Death occurred two days afterwards. At the post-mortem examination the whole left hemisphere was found to be bathed in pus. He could not ascertain whether there was a loss of bone, and on stripping back the dura no area could be found in which pus had welled through from the brain. It was extradural, and it was remarkable that the man had felt well for so long.

In the other case there was no tenderness or anything suggestive of sinus trouble; the nose was healthy and there was no evidence of pus, nor was there a raised temperature. When an exploratory incision was made in the mid-line a large amount of pus was encountered; the whole anterior wall of both frontal sinuses had been absorbed. The tissues were so much infiltrated that further operation was postponed and fomentations were applied for two days. All the diseased bone and the unhealthy tissues were then removed as far as possible. The septum between the sinuses had been partly absorbed. Several areas of bone had suggested osteomyelitis. On the left side there was no communication with the nose. On the right a large communication was made for drainage. After a few days a lateral incision was made in addition to the central one, and was kept open by external drainage. That was two months ago and the patient had now been back at work three weeks. There was little deformity remaining.

Mr. KISCH (in reply) said that the surgeon must decide on the best method of treating each individual case; no rule could safely be laid down. The essential point was to obtain proper drainage, and, if necessary, an external incision must be made. He had had three or four similar cases, and the amount of drainage secured had been adequate. The exposure at this operation enabled examination of the frontal sinus to be carried out, including its anterior wall, and the bone disease was seen to be not extensive. He did not say that he had treated osteo-myelitis of the frontal bone by this method. It was necessary to distinguish between frontal sinusitis with subsequent osteo-myelitis of the frontal bone (of which the present case was an example) and primary osteo-myelitis of the frontal bone, cases of which required more extensive external incisions.

Ulceration of Gum: Hypertrophic Laryngitis. Case for Diagnosis.

By C. GILL-CAREY, F.R.C.S.Ed.

H. S., FEMALE, aged 24, a school-teacher living in the Shetland Islands. Attended hospital, January, 1926, with a history of hoarseness for one year; general health good.

Examination of the larynx showed nodular hypertrophy of both true and false cords. No tubercle bacilli were found in the sputum.

Tissue removed from the larynx by the direct method was reported upon by Dr. Scott Williamson as: "Simple chronic inflammatory. No evidence of tuberculosis."

In March, 1926, hypertrophied tissue was removed from both cords, after exposure of the larynx with Haslinger's directoscope.

Re-admitted, April 20, 1927. States that her voice returned for six months but is now as bad as before. Has had an ulcer on her gum for the last twelve months which has not healed in spite of much local treatment.

Specimens removed from the gum and larynx were examined by Dr. Scott Williamson, who reported on both sections as: "Sub-epithelial granuloma; no sign of tuberculosis."

Wassermann negative.

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Discussion.—Sir JAMES DUNDAS-GRANT thought the condition was so suspiciously like tuberculosis that the patient should have a period of rest, with treatment on sanatorium lines, light treatment being applied to an indifferent part of the body.

Mr. T. B. LAYTON suggested that the appearance of the mucous membrane on the gum of the upper jaw proved this to be a case of lupus. Removal of a portion from the larynx had not advanced matters, and might have done some harm in such a case. He thought the condition of the mucous membrane of the mouth could be treated by diathermy, either under general or local anæsthesia. He thought the larynx should be left alone, subject to there being no suspicion of disease in the lung, and if the patient's work was not very hard she could be permitted to continue with it, under the stipulation that she took care of herself and kept quiet in the evenings. He would prescribe a tablespoonful of cod-liver oil to be taken every morning of her life.

Mr. HAROLD BARWELL said he considered that the patient was in a critical condition. She ought not to talk and he urgently advised complete rest, and, if possible, sanatorium treatment for a time.

Section of Neurology.

President—Sir JAMES PURVES-STEWART, K.C.M.G., C.B., M.D.

The Influence of the Diencephalon ('Tween Brain) on Metabolism.

By PHILIP CLOAKE, M.D. (Birmingham).

WE recognize that the central nervous system might influence the metabolism of the body through its sympathetic centres either directly or by way of the endocrine glands. There is, too, its direct influence on katabolism through muscular activity, and there is evidence that even basal metabolism may be increased by purely emotional causes [1], or decreased by cerebral tumours [2]. But it is still difficult to think of general metabolic disorders arising directly from small localized lesions in the central nervous system, since we have come to regard its functions as highly specialized.

Evidence has, however, accumulated which suggests that the hypothalamic region—embracing more particularly the infundibulum and tuber cinereum—has such a function, though how this is performed is still obscure.

My interest in the matter was aroused when, some years ago, I noticed the development of symptoms, commonly regarded as of pituitary origin, following encephalitis lethargica, in spite of the fact that Marie and Trétiakoff [3] had found no pituitary lesion in this disease. Further investigation revealed the astonishing conflict of results and deductions from experimental and pathological researches on pituitary function; a conflict which appears irreconcilable and is almost certainly due to multiplicity of unrecognized factors in the complex. The one fact that seemed to be reasonably certain was that the anterior glandular lobe in some way stimulated growth, especially growth of bones.

Erdheim [4], for example, had recorded acromegaly due to tumour growth of a cell-rest of the anterior lobe in the course of the cranio-pharyngeal duct—the embryonic duct associated with the pharyngeal origin of the anterior pituitary lobe.

Without entering here into any further discussion of this complex problem one may briefly enumerate a few of the difficulties in the way of a clear understanding of pituitary function.

(1) Recorded cases of "hypopituitarism" without pituitary lesion.

(2) Recorded cases of pituitary destruction or ablation without evidence of "hypopituitarism" [5].

(3) Cushing's view [6] that the pituitary is essential to life in the dog, supported by Blair Bell [7], and the evidence of Camus and Roussy [8], Bailey and Bremer [11], Brown [9], and Dandy [10], that dogs have survived for months after total removal of the gland.

(4) A great conflict of evidence as to which lobe of the gland was concerned in producing the alleged symptoms of pituitary lesions, for example, genital atrophy.

The effect of experimental work on the overlying part of the brain has been to establish a little more order in this chaos, to shake profoundly our views on the pituitary, and to propound a new set of baffling questions.

It was Erdheim [12] who first suggested that the distant effects of lesions about the sella turcica were due to disturbance of the function of the immediately overlying parts of the brain.

In 1891 Loeb had attributed a form of glycosuria to the diencephalon, and in 1902 Ott attributed some disturbances of temperature regulation to this region. Further researches showed that lesions or stimulation here produced slowing of the pulse [13], dilatation of the pupil [14], loss of temperature control [15], inability to

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react to fever-producing agents [16], and alteration in the amount of protein metabolism [17].

The more precise work of recent years has followed the general plan, first indicated by Aschner [18], of observing the effects upon the animal of lesions produced by puncture of the para-infundibular region of the brain.

Anatomy [19, 20] (FIGS. 1-7).

A brief review of the anatomy of this region will help to make this work clear.

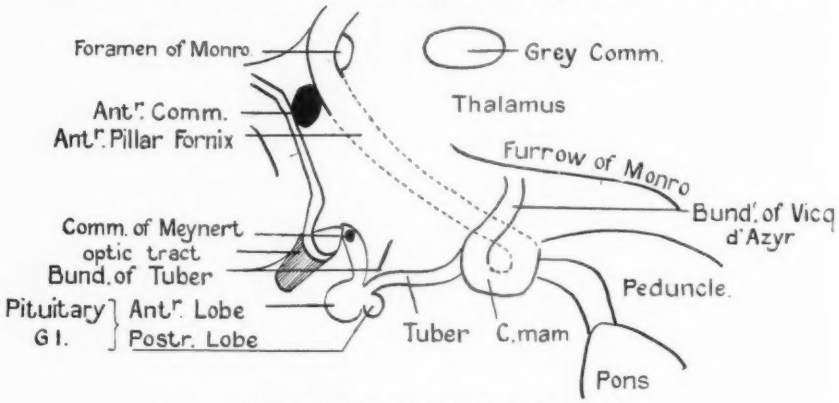

FIG. 1.—Relations of tuber (Foix and Nicolesco).

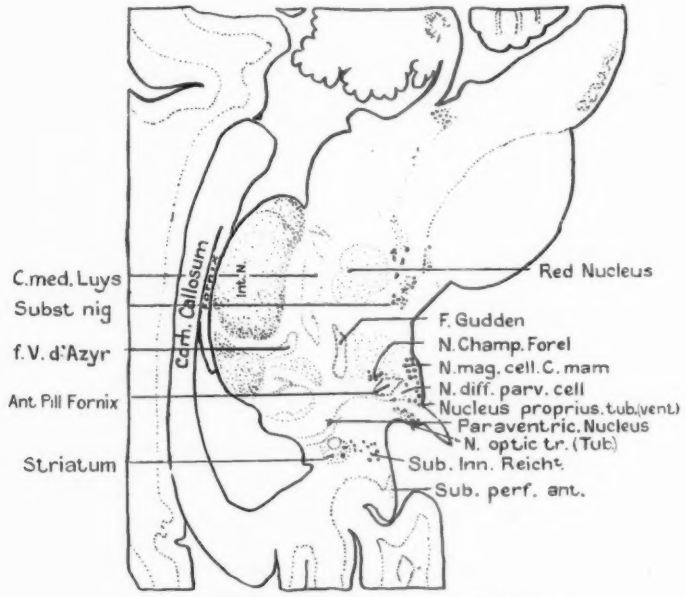

FIG. 2.—Hypothalamic region (Foix and Nicolesco).

The tuber cinereum is part of the floor of the third ventricle, beginning in front at the level of the optic commissure and ending behind at the mammillary bodies. Laterally it is not sharply demarcated, but is continuous with the anterior perforated substance, the

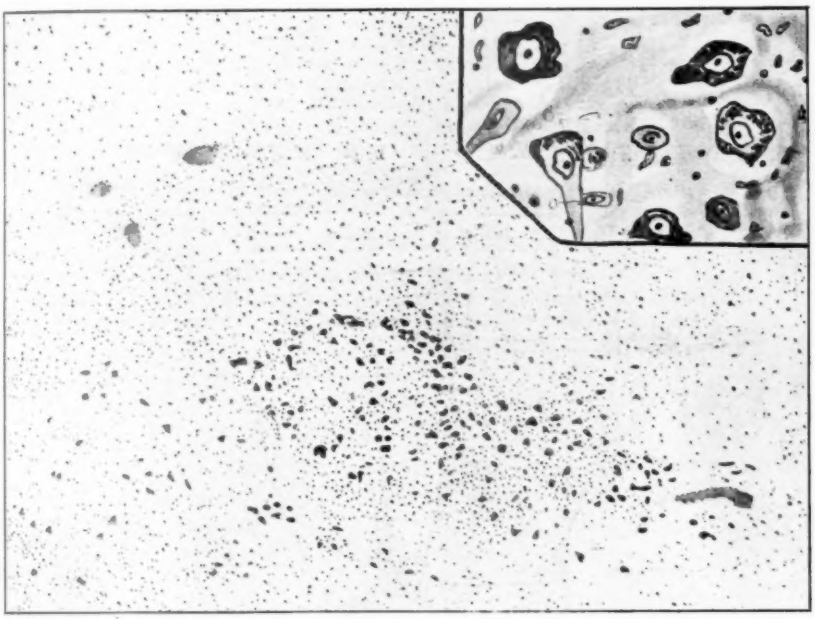

FIG. 3.—Paraventricular nucleus of tuber (Foix and Nicolesco).

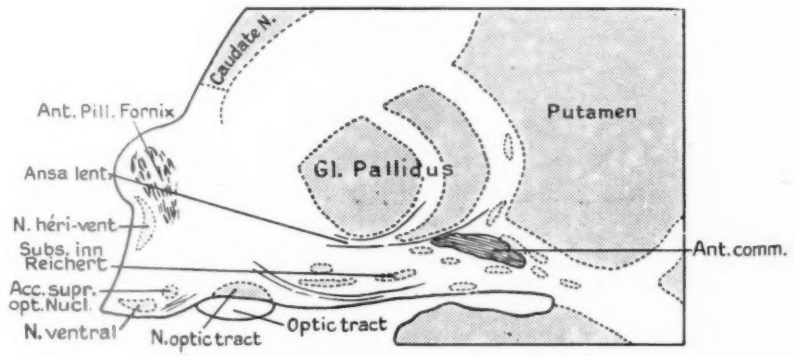

FIG. 4.—Lateral relations of tuber (Foix and Nicolesco).

sublenticular region, and the ventral surface of the anterior part of the thalamus. In the centre is the infundibulum narrowing down to the pituitary stalk.

The tuber consists of a compact mass of small cells, the small-celled nucleus of the tuber in which there are scattered groups of larger cells, with large numbers of scattered fibres and a

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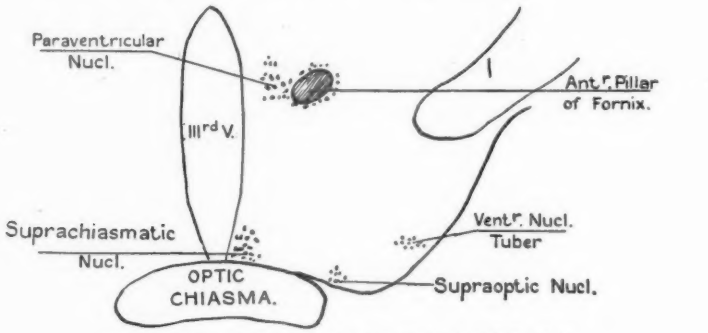

FIG. 5.—Large cell groups of tuber (Spiegel and Zweig).

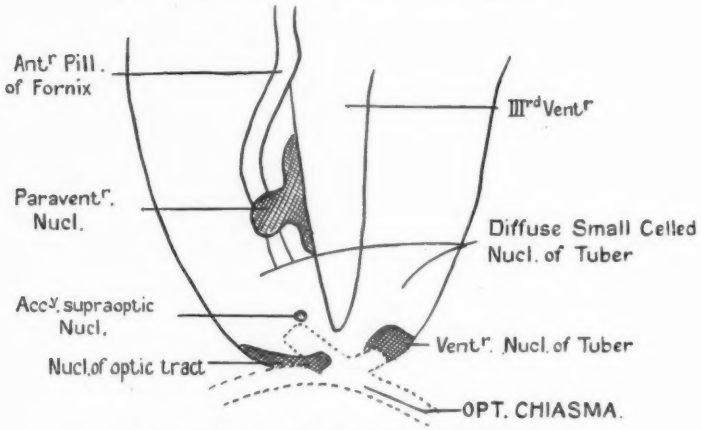
Ant r Pill. of Fornix
III rd Vent r
Paravent r. Nucl.
Diffuse Small Celled Nucl. of Tuber
Acc y. supraoptic Nucl.
Vent r. Nucl. of Tuber
Nucl. of optic tract
OPT. CHIASMA.

FIG. 6.—Diagram of tuber nuclei (Foix and Nicolesco).

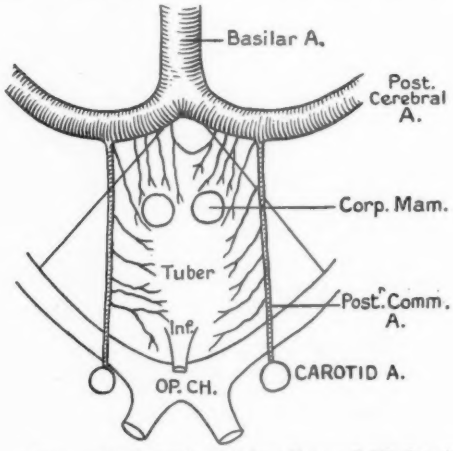

FIG. 7.—Blood-supply of tuber (Foix and Nicolesco).

few definite tracts, most of which are passing through this region from and to other areas such as the anterior pillar of the fornix, running upwards and forwards from the corpus mammillare.

The groups of large cells are: (1) *The ventral nucleus of the tuber* near its ventral surface. continuous behind with the large cells of the corpus mammillare.

(2) *The paraventricular nuclei* arranged around the anterior pillars of the fornix close to the lateral wall of the third ventricle on each side.

(3) *The nuclei of the optic tracts* on each side, subdivided by Spiegel and Zweig into the supra-chiasmatic nucleus and the supra-optic nucleus.

(4) *The accessory supra-optic nuclei*, one on each side.

The tuber has one small bundle of white fibres of its own, which runs upward and backward as far as the aqueduct of Sylvius (Déjerine) and is mainly commissural. There are also loosely-grouped fibres running into the subthalamic region.

A bundle of fibres is visible in sections of the pituitary stalk running to the posterior lobe of the pituitary (Cajal), which is said to come from the nucleus supra-opticus (Pines).

The striking points about the anatomy of the tuber are: (1) The paucity of myelinated tracts associated with it.

(2) The vast number of small cells of quite unknown function.

(3) The fact that the pituitary body receives nerve-fibres from this region as well as from the superior cervical ganglia. The region is richly supplied with blood-vessels.

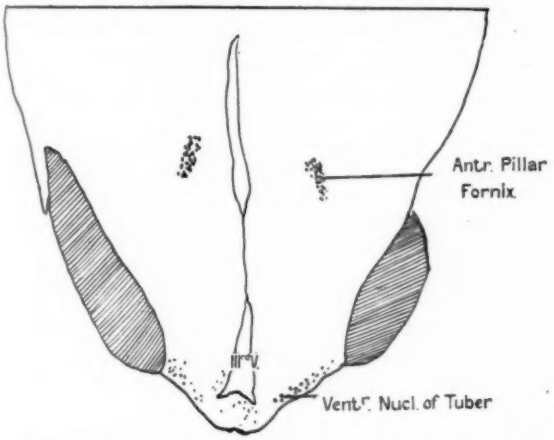

FIG. 8.—Tuber after hypophysectomy. No polyuria (Camus and Roussy).

EXPERIMENTAL EVIDENCE.

Upon this area of the brain experimental puncture has been performed. Aschner, and Camus and Roussy, operated by the buccal route, using a red-hot dental drill to penetrate the bone and puncture the tuber. With such a method if was difficult to avoid injury to the pituitary body as well, and later Camus and Roussy operated by the temporal route; so also did Bailey and Bremer, Sachs and Macdonald [21], and Dandy.

Most of the experiments have been controlled by serial microscopic examinations of the tuber and the pituitary gland.

These investigations have resulted in much evidence, for the most part consistent and confirmatory, to show that removal of the pituitary gland does not necessarily result in the death of the animal, and that its loss is not merely consistent with life, but with good health, and is at times attended with no recognizable abnormality in the animal, even of a transitory nature (fig. 8). On the other hand, extensive

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injuries of the tuber are invariably rapidly fatal, whilst slight injuries very frequently lead to abnormalities, amongst those now to be enumerated:—

(1) Polyuria and polydipsia.
(2) Glycosuria and alteration of sugar tolerance.
(3) Adiposity, with or without genital atrophy.
(4) Disturbances of heat regulation.
(5) Mental changes such as lethargy, drowsiness, or apathy.

Polyuria.

Originally recorded by Cushing as occurring in some of his operations on the pituitary body, it was also mentioned incidentally by Aschner as present in some of the animals whose brains he punctured by the buccal route.

Bailey and Bremer record its presence in twelve out of fifteen dogs with tuberal punctures. All the positive cases showed lesions in the near neighbourhood of the insertion of the pituitary without even microscopic lesions of the pituitary, whilst in the three negative cases the lesions were at some distance from the infundibulum. The polyuria began on the first and second days, reached its maximum in three or four days, and was transient (lasting six to ten days) in all but three dogs. In these the polyuria was permanent, and had the characters of diabetes insipidus in man, being to some extent controllable by injection of posterior pituitary extract. All these three dogs had other symptoms; one had progressive cachexia, hypothermia, and convulsions, while the other two developed marked obesity and genital atrophy.

In these three dogs the brain lesion is stated to have been "more important" than in those that had transient polyuria (fig. 9).

Camus and Roussy's results agreed in the main with these (fig. 10). They state that: (1) In removing the hypophysis, polyuria only occurred when the tuberal region at the level of the insertion of the pituitary stalk was affected. (2) A tuberal lesion without involvement of the pituitary determines a polyuria sometimes transient, sometimes permanent. (3) In an animal that has had transient polyuria following removal of the pituitary, a further lesion of the tuber may lead to a new polyuria, sometimes of longer duration. (4) The polyuria is from twice to six times the normal quantity.

One may add that the polyuria which occasionally follows removal of the pituitary is regarded by these authors as due to microscopic lesions in the neighbourhood of the attachment of the pituitary stalk. They have demonstrated such lesions (fig. 11).

Numerous experiments have been undertaken to elucidate the mechanism of this polyuria. It has been shown conclusively that it is not the result of nervous influences on the kidney, as Leschke [22] surmised, for it occurs when tuberal puncture is performed after both kidneys have been completely denervated [11], [24].

It is not due to any change in the constitution of the blood, so far as can be ascertained by chemical examination [23].

Cross-circulation experiments reveal no diuretic hormone in the blood; injection of urine from polyuric animals into normal animals does not produce diuresis [24].

There is reason to believe that the absence of pituitrin is not the cause, for its anti-diuretic action is variable and sometimes absent when injected into polyuric animals, and even when it is active, as great an effect can sometimes be produced by lumbar puncture or even by the administration of aspirin.

Recently, Camus and Gournay [25] have shown that uric acid disappears entirely from the urine of such animals, while the daily excretion of purin bases increases greatly, even up to thirty times the normal.

Similar findings have been recorded by Kenneway [26] in human diabetes insipidus. Camus and Gournay [27] have gone further and shown that injection of

one adequate dose of purin bases can produce a polyuria in normal animals lasting for twelve days. While these experiments are suggestive, no conclusion has been reached on this point. It seems likely that a disturbance of metabolism leads to alteration in the constituents of the blood too small to be detected chemically, but sufficient to stimulate the kidney.

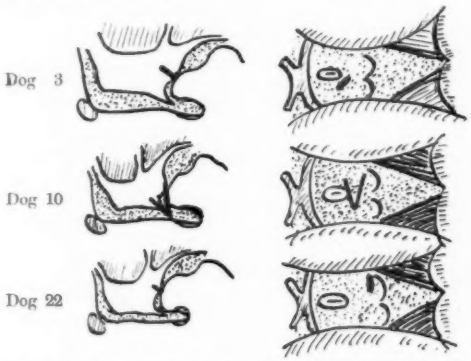

FIG. 9.—Lesions causing permanent polyuria (Bailey and Bremer).

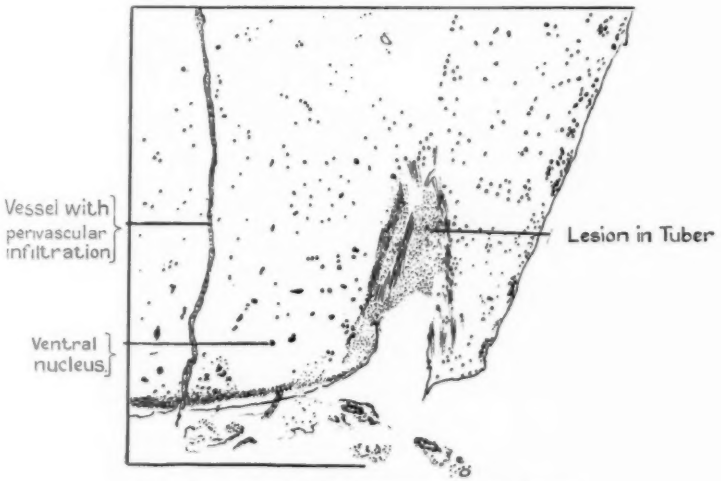

FIG. 10.—Lesion causing transitory polyuria (Roussy).

Haldane has shown that after a normal man has drunk a quantity of water, no change in the blood can be detected by chemical or other tests; but the kidney can appreciate the change in the concentration of the blood, for it responds with a polyuria.

Lesion on which the Polyuria depends.

A lesion of the tuber in the neighbourhood of the pituitary stalk has been found in all positive cases by Camus and Roussy, and has been confirmed by the other workers. The lesion may be microscopic and may amount to little more than round-cell infiltration [21].

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It would appear that polyuria is determined by lesions of the ventral nucleus in particular, and so the injury to the tuber may be quite superficial.

The depth of the lesion has no bearing on the duration of the polyuria, nor do the other nuclei take any apparent part. Roussy thinks that it is more likely to be permanent when the lesion involves symmetrically the internal parts of both ventral nuclei.

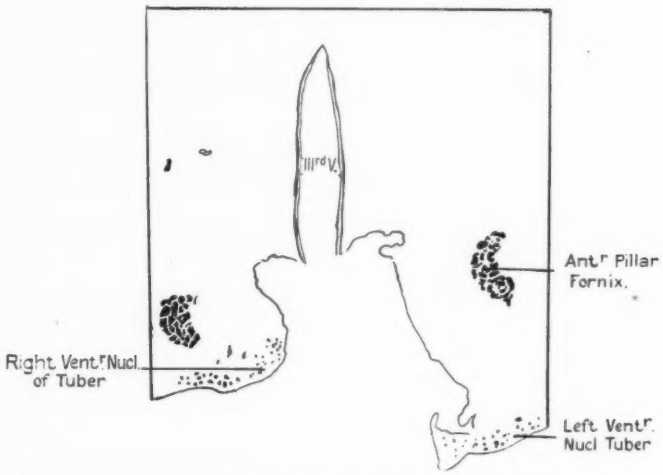

FIG. 11.—Minute lesion of tuber accidentally resulting from hypophysectomy (Roussy) (Camus and Roussy).

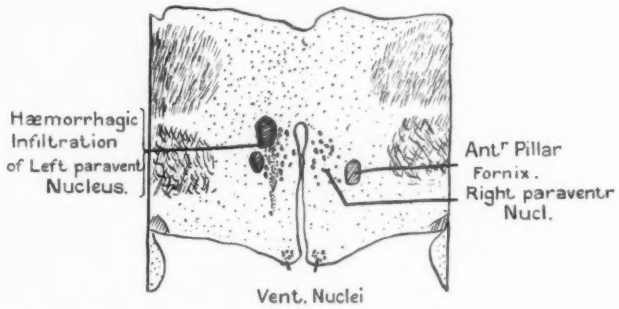

FIG. 12.—Experimental glycosuria in a rabbit (Gournay and Le Grand)

Glycosuria.

Transient glycosuria may occur after a variety of nervous lesions. Excitation of the vagus or of the cervical sympathetic chain, section of the sciatic nerve, lesions of the spinal cord, cerebellum, optic thalamus, pons and tuber, as well as the Claude Bernard puncture of the floor of the fourth ventricle, can cause it, though the last-mentioned produces it most constantly. This phenomenon appears to be the result of an excitation of the involuntary nervous system leading to glycogenolysis in the liver. It occurs rapidly after the lesion and lasts only a day or two.

After tuberal lesions in dogs it is not of constant occurrence, being recorded in

six cases out of fifteen by Bailey and Bremer, who think that its production does not depend upon so localized a region as does that of polyuria. In one animal they produced transient glycosuria with three independent punctures. Camus and Roussy's results are similar, and the experimenters agree in attributing the glycosuria to the tuberal lesion and to the finding of intact pituitary glands in such cases. Sachs and Macdonald have shown that during this transient glycosuria the blood-sugar is higher than normal; and the highest point of the blood-sugar curve occurs at the end of one hour, instead of the normal half-hour, in these cases, as it does after removal of the pituitary gland or of its posterior lobe alone.

One cannot assume from these results anything as to the existence of specific centres of carbohydrate metabolism in this region in the dog.

Camus and Le Grand [28], however, have recorded a series of observations in experiments upon the rabbit which appear to have more significance. They inserted through the front of the cranium and down to the tuberal region fine capillary glass tubes containing fatty acid, and left them *in situ*. No glycosuria occurred immediately, but after a latent period of ten to thirty days, during which time external factors remain unaltered, a glycosuria developed which differed entirely from that hitherto referred to. Not only was its appearance delayed, but it was of prolonged duration, lasting weeks or months. During this period its intensity varied greatly from day to day, the amount of sugar varying from nil to 20 grm.

Lesion upon which the Glycosuria depends.—Anatomically the lesion producing this glycosuria is one deep in the tuber, in contrast to the superficial lesion which causes polyuria. Serial sections of the brain in these experiments on rabbits showed that when glycosuria occurred the paraventricular nucleus was always injured, and in the numerous negative cases this nucleus was uninjured, independently of whether the tuber and hypophysis were involved in the lesion or not (fig. 12).

Pathogeny.—The delay in the appearance of the glycosuria suggests that it is due to the slow degeneration of cells provoked by the fatty acid. There exists a relation between the cell-group of the dorsal vagal nucleus (bulbar centre of Claude Bernard) and the paraventricular nucleus, for Lewy and Dresel [29] have shown that following the diabetic puncture of the floor of the fourth ventricle, degeneration occurs in the paraventricular nucleus. Hence it has been suggested that the latter nucleus regulates the sugar metabolism by maintaining the equilibrium of sympathetic and parasympathetic centres in the bulb. Recently it has been claimed that the paraventricular nucleus also shows changes after experimental ablation of the pancreas [30].

Adiposogenital Dystrophy.

There can be no doubt that a syndrome closely resembling Fröhlich's syndrome, if not actually identical with it, occurs occasionally after puncture of the tuber in dogs.

Bailey and Bremer record two cases of obesity and genital atrophy with permanent polyuria, and one case of testicular atrophy and polyuria in which the subjects died the fifteenth day after operation, out of fifteen dogs punctured, while Roussy speaks of having several times seen obesity of rapid development, sometimes associated with genital atrophy, with or without permanent polyuria, in dogs which had been submitted to tuberal puncture and in which the pituitary gland was microscopically normal.

Bailey and Bremer describe the gradual development of obesity, apathy and sleepiness, atrophy of the penis and sclerosis of the testicles. Though testicular atrophy may appear very soon after the puncture, the obesity cannot be due to loss of testicular secretion, for either may be present without the other [31], and the obesity is in excess of what is found after castration. At the same time there are high sugar tolerance and high adrenalin tolerance.

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Gournay and Le Grand state that they found the paraventricular nucleus always injured in dogs that had developed obesity after tuberal puncture. In the rabbit injury to this nucleus leads to glycosuria. It seems possible that obesity and high sugar tolerance are the negative aspects of the glycosuria, but these authors think that we may have here a difference in metabolism correlated with the carnivorous and herbivorous characters of the two species.

The cause of the genital atrophy can only be guessed at. Camus and Roussy judge it to be dependent on altered nervous influences, presumably through fibres that leave the central nervous system by a cranial nerve, since high section of the spinal cord does not cause it. Sachs and Macdonald record it in four (possibly five) of six dogs in which total removal of the pituitary gland, without injury to the hypothalamus, had been performed. Smith [32] also records it as a constant feature after pituitary ablation in the rat.

It would appear possible that atrophy of the testicles or ovaries is, in pituitary cases, the result of internal secretory or, at least, chemical influences [32], since it can be prevented by injection of fresh pituitary gland, and the same view may be taken of the origin of the atrophy following tuberal puncture, i.e., that it is due to general metabolic disturbances.

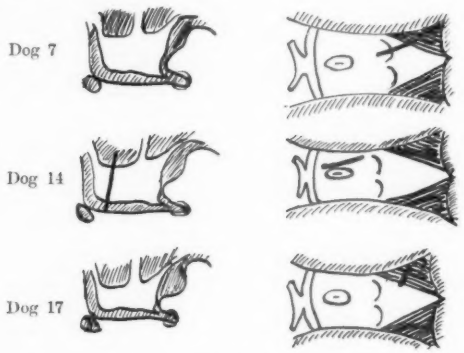

FIG. 13.—Lesions causing polypnœa (Bailey and Bremer).

Temperature Regulation.

In 1912, Isenschmidt and Krehl [15] showed that rabbits in which the brain-stem was divided at the level of the junction of the diencephalon and mesencephalon lost all power of temperature control. This did not occur if the hemispheres only were removed. Bazett and Penfield confirmed this observation in the cat, and showed that temperature control was not regained if such an animal was maintained alive for from several days up to three weeks. Rogers' observations on pigeons led to the same conclusion.

In 1914, Isenschmidt and Schnitzler [15] concluded that in the rabbit the most important central organ for temperature regulation was the tuber cinereum. Bazett and Penfield's [33] results are consistent with this, and Leschke confirmed this observation and showed that fever could not be provoked in animals with tuberal lesions by pyrogenic agents [16]. Other observers have attributed temperature control to the thalamus and corpus striatum. Kornblum [34] thinks there is no heat centre, but that there are widely distributed reflex arcs that are essential for temperature regulation.

In Bailey and Bremer's experiments one dog died after tuberal puncture with a temperature of 109° F., and one had hypothermia and died. Ott [35] recorded extreme hyperthermia after puncture of the tuber in rabbits. It appears probable that the production of hyperthermia depends on the lesion being a large one, as in the case of Bailey and Bremer's dog.

Polypnœa.

It is interesting, in view of the respiratory sequelæ of encephalitis lethargica, to note that polypnœa was recorded by Garrelon and Langlois as resulting from tuberal stimulation. It was present in three of Bailey and Bremer's dogs [11], and in all of them the lesions were large and affected principally structures outside the tuberal areas (fig. 13).

Mental Changes.

Apathy and drowsiness have been frequently recorded after puncture of the tuber [11], and there is an alteration of sex attitude in cases showing genital atrophy.

CLINICAL ASPECTS.

Modern medical literature abounds with descriptions of cases in which so-called "pituitary" syndromes" have resulted from lesions in the region of the tuber and third ventricle.

Unfortunately, suprasellar tumours are usually of such a size that the pituitary, even if not directly involved, probably has its function impaired through compression, through interference with its blood-supply, or with the channels by which its products are carried away. Any disorder resulting in internal hydrocephalus may also compress the pituitary gland, as well as the floor of the third ventricle, with which we are here concerned.

Hence, although numerous cases of metabolic disorder have been reported in suprasellar tumours [36], it is unsafe to make deductions from them as to the function of the tuber cinereum. It is only when we have small localized lesions there that records are of special interest, but we shall not ignore other cases in which tuberal lesions of any kind are present and the pituitary is recorded as microscopically of normal appearance.

Cases of Diabetes Insipidus.

It is now accepted that diabetes insipidus results from lesions about the pituitary and base of the brain, such as bullet wounds of the suprasellar region, syphilitic basal meningitis, tuberculoma of the infundibulum, pituitary tumours, third ventricle tumours; also that it follows encephalitis lethargica, cerebral malaria, and head injuries [37] (the region of the infundibulum being especially liable to trauma from general injuries to the skull).

Leschke [13] states that he saw Lewy's case of diabetes insipidus [38], in which the supra-optic nucleus of the tuber was degenerated and the pituitary intact; Lhermitte [39] also reports a case in which syphilitic meningitis had damaged the tuber and left the pituitary intact; Babonneix [40] and his co-workers record a third case with intense syphilitic basal lepto-meningitis and an encephalitis involving the pituitary stalk, the infundibulum, the tuber cinereum and, in particular, its ventral nucleus, with a normal pituitary gland.

Two interesting cases following encephalitis lethargica in children are recorded by Pincherle and Magni [41]; in both of these the tuber showed lesions, and in one there was severe degeneration of the pituitary as well. Numerous other post-encephalitic cases have been recorded, as well as several due to syphilis.

Glycosuria.

Some disturbance of sugar metabolism inducing hyperglycæmia and abnormal sugar curves is found not infrequently after encephalitis lethargica [42, 43].

In Sachs and Macdonald's paper a case of pituitary adenoma and acromegaly is recorded in which *after* the tumour had been partially removed polyuria developed, lasting three weeks, and glycosuria, lasting for ten days. The glycosuria had all the

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features of diabetic glycosuria and was controllable by insulin. Even a month after the operation oral administration of 100 grm. of glucose gave a sugar curve, the highest point of which was 0·38 per cent. at the end of two hours, with glycosuria. It appears probable that the tuber was damaged in this operation, since such a result is exceptional after pituitary operations.

Lhermitte and Roeder [44] record a case of subthalamic softening in an old man apparently determining chronic glycosuria, but the most convincing case is one recorded by Gournay and Le Grand [45] of fatal diabetic coma in a child. On serial sectioning of the tuber cinereum there was found an unquestionable localized lesion of the paraventricular nucleus with slight (eccentricity of nucleus, slight chromatolysis, vacuolization) or severe (disappearance of nucleus, vague cell masses) cell degeneration. Other cell-groups in this region showed no such change. This finding is interesting in the light of the experimental facts previously recorded.

One may recall here Lovell's statement [47] that patients dying of encephalitis not infrequently suffer from pancreatitis with cellular infiltration, or fibrous degeneration of the pancreas.

Obesity and Genital Atrophy.

Cerebral obesity is common and occurs with or without genital atrophy [31], but its exact origin is difficult to trace. Internal hydrocephalus from any cause may determine it.

Such obesity is sometimes seen after encephalitis, as in Worster-Drought's case [47], shown before this Section two years ago, a boy with behaviour disturbances, obesity and, normal genitalia, who had had encephalitis at 11 years of age. A similar case is described by Nobécourt [50].

I have never seen genital atrophy or failure of development after encephalitis but minor disorders, such as irregular or absent menstruation, are not uncommon. I have more than once seen excessive sex manifestations in boys after encephalitis lethargica, but this, I think, is more correctly regarded as evidence of weakened inhibition.

A remarkable case, described by Lereboullet [51] and his co-workers, was that of a man aged 27, who for fourteen years had remained infantile in appearance with genital dystrophy and obesity, and had an epithelioma of the third ventricle with a somewhat flattened, but otherwise normal, pituitary gland. A somewhat similar case clinically, with polyuria also, due to bullet wound of the tuber, with intact pituitary, is recorded by Maranon and Pintos.

Temperature Control.

Adie [52] states that intermittent pyrexia may be the only sign of encephalitis lethargica. Sudden variations of temperature of one or two degrees, without corresponding constitutional disturbance or apparent cause, have been recorded in association with other symptoms of tuberal lesions [53]. Only the thermometer gives indication of this temperature incoördination.

Mental Symptoms.

Lethargy and somnolence have long been recognized as symptoms of pituitary disease. Encephalitis lethargica, hypothalamic tumours and vascular lesions also cause excessive sleep. In his thesis on idiopathic narcolepsy, Adie [52] has been led to place the functional disturbance of that disease in the nuclei of the tuberal region.

Though I feel convinced that the neuronal changes associated with sleep are in the cortex, it appears to me possible that metabolic disorders of the type we have been considering could so alter the state of the cortical cells that lethargy, somnolence, or even brief, irresistible bouts of sleep might result. But as, I believe, such metabolic disorders are not seen in idiopathic narcolepsy, some other nervous mechanism seems called for, such as Adie has discussed.

Apart from sleepiness, psychic changes of affective type have been recorded in syndromes of unquestionable tuberal origin.

Roussy and Lhermitte [53] describe a case, probably neoplastic, with a complete tuberal syndrome: A girl, aged 20, with gradual onset of polyuria, obesity, and increased thirst and hunger; with amenorrhœa and thermic instability, narcoleptic and "cataplectic" (Adie) seizures, and attacks of severe headache and sickness, became gloomy and introverted, depressed, but with sudden outbursts of gaiety ending in sobbing. She lost interest in everything and fell out of sympathy with her friends and relations, a state which she appreciated and regretted but could not overcome. Her intellectual processes were unaffected—a mental condition which might well have been called a psychoneurosis, and which reminds one of the psychic disorders that follow encephalitis lethargica. The symptoms, physical and psychic, gradually regressed under the influence of X-ray treatment directed upon the tuber, and the patient, after eleven exposures of 45 to 60 minutes, recovered completely.

A somewhat similar case is reported by Le Grand [54], that of a girl aged 16. After encephalitis lethargica, together with a characteristic tuberal syndrome, she showed extreme variability of character, oscillating between exaltation and depression, anger and repentance, naughtiness and docility.

Relation of Pituitary Gland and Tuberal Centres.

Time will not allow of any adequate discussion of this most interesting question. I think the evidence adduced is sufficient to show either that the alleged effects of pituitary lesions have been wrongly attributed to that organ or that some functional correlation exists between it and the tuber.

If minute lesions of the tuber are capable of causing, as I believe they are, such widespread metabolic changes, then we must have there true metabolism-controlling centres which presumably act though the sympathetic nervous system, though we have little definite evidence of this. It is possible, however, to maintain that these centres act wholly through their power of controlling the activity of the pituitary gland, and alleged effects of tuberal lesions are primarily the result of disorganized function of this gland.

I think this latter view is untenable in view of the fact that the pituitary gland can be removed, at least in the adult animal, without producing the effects attributable to tuberal puncture; some people would say without producing any recognizable abnormality. Moreover, after the pituitary gland has been removed, puncture of the tuber can still produce some, and possibly all, of the symptoms of which we have been speaking.

Some writers contend that the pituitary syndrome is quite distinct from the tuberal. Smith [32] says that through operating by the buccal route and sucking out the pituitary gland of rats, an invariable syndrome occurs, characterized by cessation of growth in the young animal or progessive loss of weight in the adult, atrophy of the genital system and loss of *libido sexualis*, premature senescence and marked physical impairment, atrophy of the thyroids, parathyroids and suprarenal cortex and decrease in weight of the spleen, liver and kidneys: all of which can be completely or considerably repaired by the transplantation of living pituitary substance. Puncture of the tuber, however, causes polyuria, adiposity and a less severe degree of genital atrophy, the pituitary remaining intact.

I do not think, however, that the pituitary and tuber are without physiological interaction.

Collin [55] has shown that the cells of the pars intermedia undergo colloidal degeneration, and that the colloid passes by perivascular spaces and intercellular capillary interstices through the posterior lobe, pituitary stalk, and into the immediate neighbourhood of the cells of the tuberal nuclei. Abel has shown that a substance can be extracted from the region of the pituitary stalk and tuber which has the same chemical and physiological action as extract of the posterior pituitary lobe. It seems reasonable to believe that this substance is not without some influence on the function of neighbouring nuclei in the tuber.

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Indeed the posterior lobe is but a displaced part of the tuber and its displacement so far back in the scale of evolution would seem to point to a close functional relation between it and the anterior lobe and pars intermedia, and with the region of the brain whence it was derived.

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A Uterus and Vagina with Placenta *in situ*, showing extensive Carcinoma of the Cervix.

Shown by L. CARNAC RIVETT, M.Ch.

I HAVE brought this specimen before the Section on account of a point that appears to me interesting, apart from the pathological aspect of the specimen itself.

The patient was a multipara, aged 38. She was brought to me when about thirty-four weeks pregnant, in her sixth pregnancy. She was admitted into Chelsea Hospital for Women, and, as the growth was advanced, I decided to operate forthwith.

At the operation, the abdomen was opened in the middle line from the umbilicus to the symphysis pubis. The uterus was incised and the fœtus extracted. The child cried lustily at once. The cord was clamped and cut. The placental end was cut off close to the uterus and dropped back into the cavity, and the uterus quickly closed with a single continuous silk suture.

An extensive removal of the uterus and vagina then followed. The growth had commenced to invade the bladder which was opened accidentally in the process of removal, resulting in a vesical fistula. The greater part of the vagina was dissected out and clamped with the Berkeley-Bonney clamp before removal. The specimen was at once placed in a jar of Kaiserling solution.

The operation was completed without incident. The baby did very well, but the mother did not regain strength at all satisfactorily and she died six months later.

Post mortem, there were evidences of secondary growths in the pelvis and peritoneum. There was also a large mass in the left sub-maxillary region which was reported on by the Clinical Research Association as being very probably a secondary growth.

The point I particularly wish to bring to your notice this evening is the placental site. In this specimen the placenta does not appear to have begun to separate at all, and the uterine wall at the area of attachment of the placenta is about twice as thick as that of the rest of the uterus.

It is generally stated that when the uterus contracts and retracts during the third stage of labour, the inelastic placenta prevents that part of the uterus to which it is attached from retracting, so that the greater part of the uterine wall becomes thickened, the placental site being left relatively thin until the placenta separates. I do not know of any direct evidence on which the theory is based; it may be that our present view is totally erroneous.

Fibromyoma of Uterus, Rupture of Capsule, with Protrusion of part of the Tumour, with Secondary Necrosis.

Shown by S. GORDON LUKER, F.R.C.S.

MRS. A. H., aged 45, mother of three children, was sent up to the London Hospital by Dr. David, of Ongar, on September 13, 1926. She was complaining of abdominal soreness and pain of about six days' duration. The soreness started in the lower abdomen, and on the following day gave way to attacks of acute pain. There was no vomiting, and the bowels were open. The doctor gave her medicine which

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relieved the pain. He discovered the lump in the lower abdomen and sent her up to the hospital.

She was admitted on September 27. There was a history of more frequent menstrual periods during the last two years, the interval being a fortnight only, instead of a month as previously. Recently, there had been some pain just before, and on the first day of the menstrual period. On admission the temperature was 99° F. and the pulse 110.

On examination there was found to be some abdominal distension and tenderness. The uterus was enlarged and hard, rising out of the pelvis more than half way up to

the umbilicus. A second hard, smaller lump arising from the right cornu of the uterus was felt. The diagnosis of uterine fibroids was made, and the abdominal distension was thought to be due to acute, red degeneration in the second smaller tumour. During two days' observation the pulse-rate and temperature showed a small evening rise.

Operation (October 1).—The large median tumour was found to be a uterine fibroid, and the second lump near the right cornu to be a portion of the fibroid which had been extruded through a rupture or perforation in the capsule. This tumour was black and gangrenous from strangulation of its blood-supply; the small intestine and transverse colon were adherent to it. This adherent intestine was separated

without difficulty and pan-hysterectomy with removal of the appendages was carried out.

The herniated fibroid measured 3 in. in circumference, the hole in the capsule 1 in.

Microscopical diagnosis of the tumours, made at the Pathological Institute of the London Hospital by Professor Turnbull, showed that both were fibromyomata of the uterus, with focal necrosis and hæmorrhage into the herniated fibroid.

Spontaneous Separation of the Cervix in Labour.

By LEONARD PHILLIPS, M.S., B.Sc.Lond., F.R.C.S.Eng.

PATIENT, a primipara, aged 44, was first seen by me in labour at Queen Charlotte's Hospital. She had attended the Ante-Natal Department, where a breech presentation had been converted into a vertex, and finally, at the thirty-seventh week, because the head did not push easily into the brim of the pelvis, labour was induced by bougies. The pelvic measurements negatived pelvic contraction (interspinous 10½., intercristal 11½ in., external conjugate 8 in., promontory not felt).

The membranes ruptured with the onset of labour and the cervix dilated sufficiently to admit two fingers in the first six hours of labour. The pains were good, but a note was made that the cervix was rigid. No further dilatation occurred in the next twenty-one hours in spite of morphine-scopolamine narcosis. Altogether two doses of $\frac{1}{6}$ gr. of morphia, and eight doses of $\frac{1}{200}$ gr. of scopolamine were given at about two-hourly intervals, and, in addition, one dose of pot. bromide and chloral, 30 gr. of each, was administered during this period.

When examined by the resident twenty-nine hours after the onset of labour a note was made, "head well down in the pelvic brim, cervix rigid, os two fingers." So far there was no disproportion—it was purely a question of slow dilatation of the cervix.

I was first asked to see her seven hours later, i.e., thirty-six hours after the onset of labour. Her general condition then gave rise to no anxiety. In fact she walked across the Labour Ward and climbed on to a bed to be examined. There was no vomiting, no elevation of temperature or pulse, she spoke rationally, the uterus was not rigid or tender, the child was palpable per abdomen, and when a vaginal examination was made, the head was found to be in the pelvic cavity and almost to have reached the perineum. No ring of cervix could be felt, but behind the head an œdematous lip of cervix was palpable.

I remarked to the resident that an œdematous lip in front of the head was common, but an œdematous lip behind the head I had never felt before.

This examination was made seven hours after that performed by the resident, at which the whole cervix could be felt with an external os just admitting two fingers.

As the patient's condition was so good, it was decided to wait a reasonable time, and then if she did not deliver herself to extract the child with the forceps. This was subsequently attempted two hours later owing to the development of signs of fœtal distress, but when the left hand was introduced into the vagina before the left or lower blade was passed, the ring of cervix here exhibited slid out of the vagina. This was before either blade of the forceps was passed.

The subsequent history is interesting. The extraction of the child was easy and after artificial respiration the baby survived and left the hospital well.

After delivery the mother's pulse was 100 and the temperature 97° F. There was no undue hæmorrhage, and the uterus was contracting regularly.

Twenty-four hours after delivery abdominal distension developed, the uterus could be palpated high in the abdomen, the pulse was 140, the temperature normal. A

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catheter was passed in order to relieve the retention of urine. Forty-eight hours after delivery the patient looked very ill, the lochia was offensive, and the distension marked in spite of turpentine enemata and pituitrin being given.

The temperature was 101° F. and the pulse 140. The possibility of rupture of the lower segment being borne in mind, the patient was examined under anæsthesia. No rupture of the uterus was found but only a raw area on the anterior vaginal wall high up. This was cauterized and then painted with 1 per cent. violet green, the vagina was plugged with gauze soaked in flavine 1 in 500, and hourly vaginal douches of Dakin's solution ordered when the plug was removed eight hours later. At the same time a blood culture was taken. The bacteriological report was, "pure growth of *Bacillus coli*."

Two days later, i.e., four days after delivery, the patient died.

During the last two days she had a number of rigors, and marked œdema of the vulva appeared. During this period the pulse was never below 136, and the temperature never over 100° F.

An immuno-blood transfusion was given but it did no good as the patient was by this time dying.

There are many interesting features in this case which might form a basis for discussion or criticism: (i) When the patient was examined twenty-nine hours after the onset of labour, the cervix was intact, the os two fingers dilated and the head engaged in the brim. At this time there was no other apparent difficulty beyond a rigid, slowly dilating cervix. Seven hours later, the head was on the perineum and no cervix could be felt except the edge of an œdematous lip behind the head. There is no doubt that this lip of cervix was the anterior edge of a cervix already separated, pushed backwards and compressed against the sacrum by the fœtal head, and that only when the head was raised up by the left hand preparatory to passing the lower blade of the forceps was the imprisoned cervix able to escape. The value of patience in cases of slowly dilating cervices is known to us all—but in this case it had a disastrous result; nor could the disaster be inferred from the patient's condition, which was excellent even after the cervix had separated.

(ii) The case illustrates the limitations of ante-natal treatment.

In spite of external version and induction of labour in the ante-natal period, a difficulty arose in labour which could not possibly have been anticipated and corrected before labour began, but only dealt with after its development during labour. When ante-natal supervision is perfect there will still be many dangers in parturition.

(iii) The case teaches two lessons: (*a*) That in cases of scopolamine-morphine narcosis, especially when rigidity of the cervix is present, it is essential to make frequent vaginal examinations.

If before the separation of the cervix, this patient had not been left seven hours without examination, the commencing separation of the cervix might have been recognized. We teach students to make very few vaginal examinations, to limit the number to two, and, if possible, to avoid examination altogether. In actual practice, however, especially in our own private practices, we frequently make more than two vaginal examinations, an examination being made whenever we think there is a good reason.

This is probably a wiser practice and in cases of markedly rigid cervix it is no doubt very important to make repeated examinations, in fact, it might with advantage be done every time the scopolamine is repeated. Only in this way can the progress of dilatation be watched, danger detected and disaster averted.

(*b*) That in primigravidæ over forty years of age with rigidity of the cervix, especially when the membranes rupture at the outset of labour and progressive

dilatation does not occur even with scopolamine-morphine narcosis, Cæsarean section should be considered early in labour and is probably the safest method of delivery in the interests of the mother. Some elderly primigravidæ have short and easy labours, but a case such as that here described would have had a better chance had the patient been treated by Cæsarean section within twenty-four hours of the onset of labour.

Another point of interest is the cause of death. There was no rupture of the uterus and no peritonitis. The patient developed a pure *Bacillus coli* septicæmia, the result of infection of the lacerations of the genital tract—an infective condition very uncommon in obstetrics but usually fatal when it does occur. The rapid pulse-rate associated with low pyrexia suggested little resistance to the infection. Her temperature was never more than 101° F., and in the last two days of the illness it was never above 100° F.

When first examined the cervix measured 5½ in. in diameter, with an external os of 1½ in. Its substance was thick and dark with effused blood, but the periphery was thinned and frayed as though it had been subjected to prolonged pressure. It had a slightly offensive odour but was by no means necrotic. The specimen has shrunk in the process of preservation.

Discussion.—Mr. J. P. HEDLEY said that as such cases as this were so extremely rare, there seemed no need for frequent vaginal examinations in order to make sure that separation of the cervix was not occurring. Death in this case was due to an accidental infection which might have occurred after some quite ordinary laceration.

Dr. W. H. F. OXLEY said that about the year 1900 when knowledge as to pituitrin treatment was in its infancy a friend of his, writing on this subject for his M.D. thesis, gave a large dose of pituitrin to an elderly primigravida with the result that a ring of cervix was torn away and delivered in front of the child's head. The mother made a good recovery. He had not heard of other similar cases.

[*May* 20, 1927.

A Case of Repeated Tubal Pregnancy.

By EVERARD WILLIAMS, M.D.

ON August 19, 1925, L. D., a married woman, aged 24, was sent up to the hospital out-patient department by her doctor on account of a thick, yellow, vaginal discharge, of some nine months' duration, and persistent backache for about the same length of time.

Menstruation was normal, and her obstetric history was that there had been one pregnancy three years ago, terminating in the birth of a male child after a normal labour.

On examination the only abnormal condition discovered was a cervical erosion. This was painted and a douche prescribed; she was requested to attend for weekly treatment.

Three weeks later she was examined again, as she still complained of the backache, and some sharp pains on the right side of the abdomen. On pelvic examination there was felt, in the region of the right appendages, a tender, soft, pulsatile swelling which was not present on the previous examination. The breasts were then examined, and no sign of activity detected; no menstrual period had been missed. A diagnosis of early tubal gestation was made; she was admitted forthwith and operation performed next morning.

In the ampulla of the right Fallopian tube there was an early gestation and this portion of the tube was removed. The recovery was uneventful, and I did not see her again until April 26, 1927, when she came up with a letter from her doctor

containing a statement to the effect that she had the classical signs and symptoms of a tubal pregnancy. On examination a firm, insensitive, retort-shaped swelling was found in the region of the left appendages, suggesting a tubal mole.

She was admitted the following day, and operated upon three days later. The greater part of the left Fallopian tube and broad ligament were considerably damaged, and the tube was accordingly resected, as it did not seem a suitable case for salpingostomy.

The opposite appendages were then examined; the portion of tube which remained, also the ovary, appeared quite healthy. As she and her husband were both anxious to have another child, preparation was made for opening the tube, but I refrained from doing so until I had further opportunity for reflecting on the advisability of taking this course. The patient made a good recovery, and has just left the hospital.

Since having had the patient under my care I have examined the literature of extra-uterine pregnancy during the last three years, and I find there are recorded no less than fifteen instances of bilateral or repeated tubal pregnancy. This number includes two instances of pregnancy repeated in the same Fallopian tube, while reference is made to the case of Essen-Möller, in which four ectopic pregnancies occurred in the same patient.

After further reflection I have formed the opinion that it would not be advisable to perform salpingostomy in this case, but I should like to have the opinion of Members of the Section on this point.

The other matter of interest to me is that of the ætiology of ectopic pregnancy. Dr. Lockyer was only able to collect forty-one cases of ovarian pregnancies up to 1917, yet in the *Index Medicus*, during the last three years, not including the year 1927, there are no less than thirteen instances recorded. This indicates that, since cases in general are more freely recorded and indexed, the condition is obviously more frequent than has been generally supposed.

Any theories advanced in explanation of ectopic pregnancy must therefore include discussion of ovarian gestations and repeated tubal pregnancies, as well as of the more usual tubal condition. They should at the same time explain why embedding normally takes place in the body of the uterus, a fact which calls for elucidation, before the reason why embedding occasionally takes place in some other situation is discussed. The existing theories as to salpingitis, tubal hypoplasia, internal migration and others fail to do this. I will therefore enunciate a new theory which I believe will explain the observed facts, though I have as yet but scanty evidence in support of the theory.

Numerous observers have called attention to the decidual reaction which is noted in the connective tissue of the surrounding parts in both ovarian and tubal pregnancies, but so far as I have been able to discover they have all concluded that the decidual reaction is the result of the ectopic pregnancy. There are several recently published papers dealing with decidual reaction, since Adler first demonstrated that decidual cells are formed in the pre-menstrual mucosa. Apart from the body of the uterus the formation of decidual cells has been observed on the peritoneum, in the ovary, in a subperitoneal fibroid, in the Fallopian tube, and in more distant situations.

The prevailing view seems to be that this formation is initiated by, and its development controlled through, the corpus luteum; and it appears to be established beyond doubt that the connective tissue cells of the ovary, the Fallopian tube, and in exceptional circumstances other connective tissue cells, as well as those of the endometrium, have the power of developing into decidual cells, under a suitable stimulus. The old view that decidual cells only formed under the stimulus of pregnancy is obsolete, and not in accordance with the observed facts.

No explanation has been made as to why decidual reaction occasionally takes place in these extra-uterine situations. I have none to offer, but I do suggest that it is the formation of decidual cells which is the primary event, and that this determines where the ovum will be embedded. Also, that there is a chemical affinity between the ovum and decidual tissue, and that in whatever part of the genital tract the ovum first meets with decidual tissue, there it will embed itself. This hypothesis will explain the occurrence of ectopic pregnancy in the various sites where it has been observed, and it will also explain why embedding normally takes place in the body of the uterus.

Dr. T. W. EDEN said that the suggestion Dr. Williams had made with regard to the implantation of the fertilized ovum was a very interesting one. He remembered that many years ago Dr. Berry Hart, of Edinburgh, advanced the view that the ovum could only be implanted on a connective tissue surface, and the denudation of patches of the menstrual endometrium was a necessary preliminary to implantation in the uterus. This theory resembled that suggested by Dr. Williams in attributing an all-important rôle to connective tissue. He thought Dr. Williams should define more clearly what he meant by a "decidual reaction." The formation of large connective tissue cells was only one of a series of changes which made up the transformation of endometrium into decidua. If there was any special chemical affinity between these "decidual" cells and the fertilized ovum, then the exfoliation of overlying epithelium would appear to be a necessary preliminary to embedding.

[*May* 20, 1927.

The Internal Secretions of the Ovary.

By A. S. PARKES, M.A., Ph.D., D.Sc.

(*Beit Memorial Research Fellow.*)

THE solution of the type of problem found in studying the internal secretions of the ovary is most satisfactorily sought by experiment, and since the lower mammals have to be used for this type of work, it is on their reactions that our knowledge of ovarian activity mainly depends. At the same time, however, ovarian activity in the human subject must obey similar laws, and with the aid of clinical observations, experimental work on the lower mammals may be made to throw much light on the problems associated with the human species.

The ovarian cycle, consisting of the periodic maturation of ova and the consequent production of corpora lutea, is strictly correlated with changes in the accessory organs. This synchronization is so affected that the right thing is happening in the accessory organs at the right time. At ovulation changes occur which characterize the period of œstrus, this being the time, and the only time in lower mammals, at which the female will receive the male. Immediately following this phase, and correlated with the early growth of the corpus luteum, the uterus makes preparations for the reception of the fertilized ovum. In the absence of fertilization, uterine degeneration sooner or later sets in, to be followed by a period of rest (diœstrus or anœstrus) before the next occurrence of œstrus. The œstrus phase of the cycle associated with the maturation and rupture of follicles is commonly known as the "follicular" phase, whereas the stage associated with the development of the corpus luteum is called the "luteal" phase. The entire pubertal life of the uterus may thus be said to consist of a series of preparations for the reception of fertilized ova, and in the absence of pregnancy the cycle goes on indefinitely during reproductive life.

The actual degree of preparation which is made for the reception of fertilized ova varies greatly in different species. In the mouse and the rat the preparation is very slight, and consists only of certain changes which are associated with the maturation

of the Graafian follicle. If fertilization is not effected, the cycles immediately start again, and no further time is occupied in preparation for the reception of ova. In other mammals, however, such as the guinea-pig and the cow, the changes associated with ovulation are only the beginning of the active phase of the cycle. After ovulation the corpus luteum persists for a much longer period than in the mouse, and a phase of the cycle is found which is dominated by the corpus luteum. In this phase of the cycle, which follows immediately after ovulation, extensive preparation for pregnancy is made. Thus, in the guinea-pig decidual tissue is found to be present in the uterus soon after each ovulation, irrespective of whether or not fertilization has occurred. This development of the uterus proceeds until the corpora lutea undergo involution, and then involution of the uterus also begins. The type of cycle represented by the guinea-pig may show further elaboration and this leads to the condition of affairs which is found in animals such as the dog and the rabbit. In these and other animals the stage of the active phase of the cycle associated with ovulation is purely a minor one and the luteal phase is accentuated until a semblance of pregnancy is found, even in the absence of fertilization. The condition of affairs which is found after ovulation in these animals is known as pseudo-pregnancy. The uterine changes associated with the development of the corpora lutea are very considerable, and in addition, changes characteristic of pregnancy also take place in other accessory reproductive organs, such as the mammary glands. At the end of pseudo-pregnancy, brought about by the involution of the corpora lutea, many phenomena which are characteristic of true parturition are found. Thus, in the dog, lactation may actually start at the end of pseudo-pregnancy, and in the rabbit a nest is made as though for the reception of young. Pseudo-pregnancy represents the highest state of development of the accessory organs of reproduction in the absence of true pregnancy.

As regards these lower mammals, the cyclic changes which take place are fairly definitely known, and the significance of the changes in one species, as compared with those in another, is pretty well understood. Controversy, however, still surrounds the problem of correlating the cyclic changes in the human female with those which are found in the lower mammals. Much evidence, however, goes to show that the luteal phase of the cycle is dominant in the human subject, and that menstruation represents degeneration of the uterus consequent upon the failure to conceive. On this view the human menstrual cycle may be explained as follows: Ovulation is commonly supposed to occur about thirteen to fifteen days after the beginning of the previous menstruation. The subsequent development of the corpus luteum brings about the pre-menstrual development of the uterus, and the involution of the corpus luteum after about ten days' functional development leads to the retrogressive uterine changes which result in menstruation. On this theory, therefore, the pre-menstrual development of the uterus is preparation for the reception of a fertilized ovum, and is under the control of the corpus luteum; menstruation itself may almost be said to be the abortion of an unwanted decidua.

The various types of sexual cycle which are found in mammals have now been briefly indicated, and the means by which the ovarian cycle is so exactly correlated with that occurring in the accessory organs must be considered. Experimental excision has made it quite certain that the presence of the ovary is essential for:—

(*a*) The early development of the accessory organs.
(*b*) The cyclic activity of the accessory organs in the absence of pregnancy.
(*c*) The maintenance of pregnancy and the development of the mammary glands.

This points to the clear deduction that those changes result from ovarian activity. In addition, the fact that transplantation has not the same inhibitory

action as ovariotomy suggests most strongly that the control by the ovary is endocrine in nature.

The third and most conclusive test of internal secretion, namely, the extraction of the active principle, has, however, only been performed definitely as regards one of the numerous functions of the ovary. For many years various workers have been preparing extracts of ovary with a view to producing in the ovariotomized animal changes similar to those which are found in the intact female. Particularly, their efforts were directed to obtaining extracts capable of producing symptoms of œstrus after removal of the ovaries. The early efforts were, however, greatly retarded by the lack of any easy and conclusive means of testing for the occurrence of œstrus, and it was not until the elaboration of the vaginal smear technique in mice, rats and guinea-pigs, that much headway was made with the extraction of ovaries. This technique, however, has now made possible the easy and rapid testing of any extract for œstrus-producing properties. As a result of this advance in technique a definitely œstrus-producing extract of ovary was soon prepared, the credit for a first conclusive demonstration belonging to Allen and Doisy. Since their first work was published in 1922, a great deal of work has been done on the œstrus-producing hormone of the ovary, especially as regards its purification and physiological action. The original source of this hormone was liquor folliculi, and the fact that the œstrus-producing hormone of the ovary was originally detected in the Graafian follicle lent strong support to the theory that the maturation of the follicle was the direct causative factor of the œstrous changes in the accessory organs. Since that time, however, the œstrus-producing hormone has been detected in the stroma tissue of the ovary, in the fluid which is found inside hollow corpora lutea and in the placenta, in amounts comparable with those found in the follicle. It cannot be said, therefore, on the evidence of its presence there, that the œstrus-producing hormone is essentially elaborated by the maturing follicle. We have, in fact, definite evidence that this hormone is not essentially elaborated by the follicle, and that the maturing follicle is not the essential factor in the causation of œstrus. I refer, of course, to the numerous experiments on X-ray sterilization of the ovary which have shown that the œstrous cycle still persists after the entire obliteration of the Graafian follicles. As a result of the preparation of the œstrus-producing principle of the ovary, it may be said that the control of the follicular phase of the cycle has now been partially determined, but we are little further on in locating the internal secretions associated with the corpus luteum. The functions of the corpus luteum have been roughly indicated above, and they may be summed up as: (1) The sensitization of the uterus after ovulation; (2) the maintenance of pregnancy; (3) inhibition of œstrus and ovulation during pregnancy, and (4) the development of the mammary glands. That these functions are probably performed by means of internal secretion on the part of the corpus luteum has been found in the same way as the hormonic control of œstrus was determined, namely, by excision and transplantation. Extracts of corpus luteum have, however, been far less successful in imitating the effects of the intact organ than have the œstrus-producing extracts, and the only function of the corpus luteum which has so far been successfully simulated by extraction and injection is that of œstrus inhibition. Within the last two years it has been found possible to stop the occurrence of œstrus in the normal female rodent by the injection of extracts of corpus luteum. Whether this same extract will perform the other functions of the corpus luteum has not yet been determined, but on general grounds it seems improbable that one single substance can perform the varied functions of the corpus luteum.

We are in a position, therefore, at the present moment, to say that we have at our command the œstrus-producing hormone of the ovary and the œstrus-inhibiting principle—the latter in an extremely impure state. What clinical use

can be made of our present ovarian extracts? If, as seems most probable, the human cycle is mainly dominated by the corpus luteum, and the phenomena associated with pregnancy (as among other mammals) are entirely under the control of the corpus luteum, it does not seem that there is much scope for the clinical use of the œstrus-producing hormone. Actually, however, the situation is not so unpromising as would appear at first sight. The general nature of the reproductive cycle suggests that the œstrus-producing hormone and the corpus luteum act in many ways in opposition to each other, and experimentation has shown that the injection of the œstrus-producing hormone during the time when the corpus luteum is dominant, namely, during the luteal phase of the infertile cycle, during pregnancy, or during lactation, may override the domination of the corpus luteum. Thus, animals may be brought into œstrus, when œstrus would not otherwise occur, and pregnancy may be terminated, when the corpus luteum is overridden by the injection of the œstrus-producing hormone. It would seem, therefore, that a possible clinical use for the œstrus-producing hormone could be found in injection for the purpose of overriding a persistent corpus luteum, when a persistent corpus luteum is leading to an undesirable condition.

Another line of work also suggests a promising scope for clinical work. There is some reason to suppose that the œstrus-producing hormone is responsible for the maturation of Graafian follicles, as well as for the other phenomena associated with œstrus, and, if this is so, the hormone must be supposed to have a stimulating action upon the Graafian follicles. Further, since the maturation of the Graafian follicles is essential for the subsequent production of corpora lutea, it is highly probable that pathological conditions brought about by deficient activity of the ovary resulting in the absence of maturing Graafian follicles and of corpora lutea might be remedied by stimulation of the ovary through the injection of the œstrus-producing hormone.

As regards the hormone or hormones of the corpus luteum it is clear that they would provide a valuable clinical weapon in cases in which deficiency of the corpus luteum was indicated, as, for instance, in some cases of amenorrhœa, premature birth and mammary deficiency. As pointed out above, however, such extracts of corpus luteum as have been found to possess any activity, are so far insufficiently pure and insufficiently studied for clinical use.

Cases in which the œstrus-producing hormone (for which the name "œstrin" has elsewhere been suggested) has been used clinically, include those of amenorrhœa, sterility, and delayed parturition. Some time ago Allen, Pratt and Doisy reported that they had induced pre-menstrual symptoms by the injection of the hormone, but the small doses used by them and the indefinite nature of the symptoms produced suggest that their results cannot be considered of much value. More recently, positive results have been obtained in cases of sterility and amenorrhœa. In this connexion recent work on the conditions governing parturition is of interest. The researches of Marshall and Dixon suggest that parturition is brought about by excitation of the pituitary gland at the end of parturition, and further, that this excitation results from ovarian activity, which commences upon the retrogression of the corpus luteum. It would seem, therefore, that injection of the œstrus-producing hormone during pregnancy in sufficient amounts to override the persistent corpus luteum would result in premature parturition being brought about in the human subject, as it has been found to be in lower mammals.

A further type of pathological condition, in which ovarian therapy is indicated, comprises cases of infantile uteri, but it is doubtful whether the œstrus-producing hormone is of any definite value in such cases. The initial development of the accessory organs of reproduction is, of course, known to depend upon ovarian activity, and it has been found experimentally that the infantile uterus of the

immature rodent can be made to undergo sudden hypertrophy as the result of œstrin injection. This hypertrophy, however, is merely the result of the effort of the immature uterus to come into œstrus, and appears to be an evanescent effect. We have, therefore, no direct evidence that the œstrus-producing hormone is the same factor as that responsible for the original pre-pubertal development of the uterus, and no evidence therefore that injection of the œstrus-producing hormone would have any specific therapeutic effect in cases of infantile uteri.

I am afraid that this review of our present knowledge of the internal secretions of the ovary can hardly be considered as a satisfactory record of work accomplished, especially as regards clinical applications, but we have at least progressed in recent years, and though our knowledge of ovarian activity relates largely to the lower mammals the future should see it applied to the human subject with striking success.

Professor BLAIR BELL said that he himself had always taken the greatest interest in comparative histological and physiological studies of the female genitalia, and had endeavoured to apply observations made on the lower animals to human phenomena. This, however, was not always directly possible, for there were in some respects considerable differences in structure, and, therefore, in function. Nevertheless, the main features were doubtless similar among placental mammals.

Pseudo-pregnancy was a very common and interesting condition that occurred probably only in animals under domestication and experimentation, and in man. He could not discuss at length the histological appearances found in the uteri of animals in these circumstances, but he would like to emphasize that the corpus luteum not only persisted, but might even undergo hypertrophy. This was of great importance, for it was correlated with psychic and constitutional disturbances similar to those seen in true pregnancy. Professor Blair Bell then recounted his observations of pseudo-pregnancy in a terrier-bitch; six weeks after œstrus she would make a nest for the expected puppies that would never appear, her mammæ would enlarge and "milk" would be secreted. At this time, although a great pet, she lost much of her interest in human beings. In rabbits the condition could be produced experimentally and almost at will.

With regard to what might be called the physiological antagonism between the follicular mechanism and the corpus luteum, he believed that Leo Loeb was the first to demonstrate the cyclical correlation between the two structures. That was a discovery of the first importance; it formed the basis of much subsequent work in regard to œstrus and menstruation.

With regard to the effect of X-rays on the ovary and especially in regard to the statement that in mice destruction of the follicles did not inhibit the œstrus, he himself had shown many years ago that in the rabbit the uterine function might be maintained by the transplantation of the central portion of the ovary which contained only interstitial cells. In adult rabbits, the ovaries of which were chiefly composed of interstitial cells, it was quite easy to resect the follicles which were situated entirely on the surface of the organ.

He asked all those now working on the active hormone of the follicular secretion, not to neglect the opportunity of studying the effect of this substance on the other organs of internal secretion. He had shown some fifteen years ago that the ovary was intimately connected with the thyroid and pituitary body, and the function of these organs formed a definite complex which he had described as the "sex-complex." It might, therefore, be said that the effect of this correlation of secretions was not only local, but also general, psychical and physical.

[*June* 17, 1927.

Radiogram of a Nagele's Pelvis.

By W. GILLIATT, M.S.

THIS is a radiogram of the pelvis of a young woman, aged 22, who consulted me in February, 1926, when twenty weeks pregnant for the first time. She had been operated upon in Liverpool in May, 1924, for a renal calculus and had been told she had an oblique pelvis, which had been discovered when the urinary tract was X-rayed. She subsequently sent me this print which shows very well the absence of the right ala of the sacrum.

50 Gilliatt: *Radiogram of a Nagele's Pelvis*

The following points are of interest to this Section and are my reasons for showing the print :—

(1) The external pelvic measurements would have passed for normal. They were: Interspinous 9 in., intercristal 9 in., external conjugate 7½ in.

(2) The true pelvis had little or no concavity on the right side, which felt like a straight wall of bone shutting off a large part of that half of the pelvic cavity. This could be readily

appreciated on vaginal examination and was confirmed at the time delivery was carried out by Cæsarean section.

(3) The method of delivery was never in doubt, as a living child could not have been born except by Cæsarean section.

The operation was performed on June 14, 1926, and the patient made an afebrile recovery.

Mr. J. BRIGHT BANISTER asked what was the position in which the child lay, as a case of the same deformity which had come under his notice recently at Charing Cross Hospital had demonstrated the importance of the point.

In this case there was a marked reduction of the available space on the right side of the pelvic brim, and he (Mr. Banister) had considered the advisability of Cæsarean section. But on the patient's admission to hospital it was found that the child was lying with the occiput to the left and in front, and this mechanically advantageous disposition of the head had allowed of a natural delivery within a few days of full time.

Some Results of Myomectomy undertaken in the hope of Curing Sterility.

By J. Bright Banister, M.D., F.R.C.S.Ed.

Much has been written concerning the relationship which may exist between the presence of fibroid tumours in the uterus, and sterility. The presence of such tumours has been regarded by some as a cause of the infertility of the patient in whom they have been discovered, but by other authorities these fibroids have been regarded rather as developing because the patient has had no children. It is at any rate certain that the two conditions commonly co-exist.

If after an operation for the removal of fibroids a patient conceives and bears children, it is an interesting clinical fact, but there is no evidence that such operation has produced a more favourable local condition and so has contributed to the happy result. If, however, a series of such cases be produced, I venture to think that it might be conceded that there may be some relationship between the removal of the fibroids and the subsequent pregnancies.

In the cases which I report to-night there was but little indication for the removal of the fibroids, beyond the facts that the patients had fibroids in their uteri and had no children. All of them were extremely anxious that pregnancy should occur and were perfectly willing to undergo the discomfort and danger inseparable from an abdominal operation in order that they might have an increased chance of attaining their desire.

In two of these six cases the patients were advised against undergoing operation. I feel, therefore, that perhaps I am justified in bringing the cases before the notice of this Section this evening and in suggesting that the presence of fibroids in the uterus of a woman who has been sterile for some years and who is very anxious to become pregnant, may in itself be a justification for their removal.

Case I.—Mrs. W., aged 34 and married four years. She gave a perfectly normal menstrual history and stated that there was no difficulty or pain on coitus. She had worked hard as a V.A.D. during the War and had never been off duty through illness. She complained of severe sciatica for which she underwent various kinds of treatment without any great success resulting.

On examination the pelvis was found to be filled by an irregular, hard mass indistinguishable from the uterus and somewhat tender on the left side, which was the side upon which she had had sciatica.

The husband was found normal on examination.

In November, 1923, I resected a large blood-cyst from the left ovary and enucleated four fibroids from the uterus, the largest being the size of a cricket ball. One of these fibroids grew in the posterior wall and I therefore ventrally fixed the uterus with one stitch.

In May, 1925, she became pregnant and after a normal pregnancy the baby was born under the care of Dr. Charsley, of Sydenham, without any undue anxiety.

Case II.—Mrs. G. H., who was referred to me by Dr. Hignett, in March, 1925. She was aged 39, and had been married ten years. There was no abnormality of menstruation. A year previously she had been examined by a gynæcologist and a diagnosis of fibroids was made. Operation was not advised, as it was assumed that it would not affect the sterility. The husband was possessed of normal secretion.

On May 16, 1925, I removed five fibroids from the uterus, the largest being the size of a duck's egg and occupying a position low on the right side. It displaced the uterine artery forwards.

In October of the same year she became pregnant, and when I saw her in January, 1926,

everything appeared to be progressing normally. Unfortunately she had a stillborn child at the end of May after the membranes had ruptured some days previously.

Case III.—Mrs. R., aged 26, who came to the Out-patient Department at the Chelsea Hospital for Women, complaining of sterility after five years of married life, but of no menstrual irregularity. She was found on examination to have an irregularly enlarged uterus reaching to within 2 in. of the umbilicus.

On September 29, 1922, I enucleated eight fibroids ranging in size from that of a cherry to that of a cricket ball and found in various situations. I heard nothing of this patient until two months ago, when she sent a relative to see me, who told me that she (Mrs. R.) had had two children—one in November, 1923, and the second in August, 1926.

Case IV.—Mrs. C., aged 31 when I saw her, who had been married seven years without any children. She reported that her periods were excessive, lasting six to eight days, but that there was no pain nor any other symptom. She had noted a swelling in the abdomen for two years. In May, 1921, I enucleated seventeen fibroids from various situations, leaving the uterus in a somewhat battered condition. Subsequently Mrs. C. had two children—one in October, 1923, and the second in August, 1925.

Case V.—Mrs. A. B., whom I saw in May, 1924, complaining of sterility. She was aged 29 and had been married four years. She had no symptoms whatever, but on examination there was found a large retroverted uterus, the retroversion being produced by what I thought was a fibroid in the posterior wall. She had been advised that the condition had no bearing on the sterility and was best left alone. In July I removed this fibroid, which was the seat of advanced degeneration, and ventrally fixed the uterus with two catgut stitches.

In July, 1926, she bore a full-time healthy child without untoward incident.

Case VI.—Though not identical with the foregoing this case is at least germane, from the fact that it concerns the possible effects of fibroids on pregnancy.

Mrs. D. was referred to me by a doctor because she had a history of three miscarriages at the third or fourth month and was now seven months pregnant, with a tender circumscribed tumour in the middle line just below the umbilicus. This tumour seemed part of the uterus and was considered to be a fibroid undergoing red degeneration. No interference was advised, but in four weeks the patient gave birth to a stillborn premature fœtus. After this she was advised to undergo removal of the fibroids in the hope that a future pregnancy might result in a more fortunate issue. I removed four fibroids from the uterus at the Freemasons' Hospital, and in nine months' time she became pregnant again. She went to full time and was in due course delivered of a male child, who is alive and well.

These cases will, I hope, tend to show that there is some justification for the operation of myomectomy, when it is undertaken for no other reason except that the patient is either sterile or has had repeated miscarriages.

Discussion.—Dr. EVERARD WILLIAMS asked Mr. Banister whether his cases had been investigated from the point of view of tubal patency, before the operations of myomectomy were undertaken. The frequency with which salpingitis complicated cases of fibroids suggested the desirability of making the preliminary investigation, when the operation was undertaken to cure sterility, and not because of menorrhagia or other disturbances of menstruation.

He further asked Mr. Banister whether his experience of this type of case led him to offer any explanation as to why the fibroids caused sterility. His own very limited experience led him (the speaker) to believe that the explanation was that the fibroid or fibroids acted mechanically by interfering with the patency of the Fallopian tubes; he quoted a case which supported this view.

Mr. W. GILLIATT said that he had seen pregnancy follow myomectomy and felt that it was more likely to occur after removal of a fibroid which had invaded the uterine cavity. He would be glad to know whether Mr. Banister's experience agreed with this.

Mr. BANISTER (in reply) said that the cases reported had not been examined from the point of view of patency of the tubes previous to operation. In his experience when salpingitis complicated a case of fibroids there were usually symptoms present.

As to the reason why the fibroids caused the sterility he (Mr. Banister) was unable to give any, but he felt that the mechanical factor was probably of importance. In answer to Mr. Gilliatt he agreed that invasion of the uterine cavity by a fibroid would be an increased bar to the occurrence of pregnancy, but such invasion was only noted in two of the cases under review.

Section of Odontology.

President—Mr. J. B. Parfitt, L.R.C.P.Lond., M.R.C.S., L.D.S.Eng.

The Gingival Margin.

By Professor B. Gottlieb (Vienna).

I wish to thank you for the great honour you have done me in inviting me to address your Society. I much appreciate the privilege of speaking in a country where such men as John Hunter, John and Charles Tomes and a great number of other famous men of our profession have lived and worked. You are not only celebrated historically, but also by reason of the research work of to-day. I need only mention, for example, the work on vitamins, the detection of the translucent area around the caries of enamel, and the proof of the existence of lymph circulation in the dentine and especially in the enamel, the latter being the most important discovery made by the dental profession during recent years.

This evening we have to discuss the gingival margin. When the formation of the enamel is completed, the inner epithelium, the stratum intermedium and the enamel pulp disappear by resorption, and *the outer enamel epithelium is grown together with the surface of the enamel.* An enamel cuticle formed by the ameloblasts before they disappear is not by any means always to be found. It seems that the formation of this cuticle does not take place in all cases, but that it can, more or less, calcify like other parts of the enamel. Then it cannot be clearly distinguished, especially in decalcified specimens. This cuticle, however, is rarely found. The outer enamel epithelium may be associated with some remnants of the stratum intermedium. It is really not possible to decide this question, but one can say that the main bulk of the epithelium connected with the surface of the enamel before eruption is derived from the outer enamel epithelium. The growing germ approaches the oral epithelium and the epithelium covering the enamel joins the oral epithelium. The point of junction atrophies and the tip of the tooth now projects into the oral cavity, while the angles of the junction area border upon the erupting tooth. These angles where the oral epithelium and the outer enamel epithelium connect form the gingival margin. *The outer enamel epithelium is still connected with the enamel surface, being called the "epithelial attachment." The more the tooth erupts the more the epithelial attachment becomes detached from the enamel surface, so that on the average the detachment takes place nearly up to the line between the middle and cervical third of the crown when the tooth has reached the occlusal line.* The bottom of the gingival pocket after clinical eruption is thus some distance from the amelo-cemental line and we have to correct our idea about the normal depth of the pocket. It has always been thought that the normal pocket reaches from the gingival margin up to the amelo-cemental line and we have found it nearly 1½ mm. deep. We now know that that depth has been much over-estimated, and has never been found in young persons, but in correcting this mistaken idea we cannot definitely state the depth of a normal pocket. There are two means of discovering what is the normal. Either we study a great bulk of material yielding differing results and take the average, or we take that state as normal which seems to ourselves to be ideal. The arithmetical mean found in our laboratory by Orban and Kohler when measuring 356 pockets of patients with sound epithelium was 0·67 mm. I do not think that there is any reason either from theoretical or from practical points of view to assume this or any similar depth to be the normal, regarding greater depth as

pathological. The method generally followed of drawing a sharp line between normal and pathological may have its advantages, especially for teaching purposes. But nature, as a rule, draws no such line. The normal passes over imperceptibly into the pathological without any sharp distinction. The application of the same method I believe to be advisable in estimating the depth of the pocket. We have to state what depth the ideal pocket should possess. Here we can say that there are to be found pockets of quite ideal nature which have practically no depth at all. In fact this state seems to me to be ideal. On the other hand, it is very difficult to draw any hard and fast line as to where the pathological depth begins. We find in certain circumstances depths of more than 5 mm. without being justified in calling them pathological. In an example now shown there is a depth of 5·36 mm. without any pathological changes in evidence on the epithelial surface. This depth is due to topographical conditions. It is the distal side of a lower third molar. On what does the depth of a pocket depend? It does not depend only upon the amount of detachment of the epithelium from the surface of the tooth; primarily, it depends on the atrophy of the gingival margin. When the gingival margin follows the detachment step by step, then the depth of the pocket remains the same. When, on the other hand, the gingival margin does not follow, then the depth increases. In our case, the gingival margin at the distal side of the lower third molar could not follow the bottom of the pocket. It was kept in its previous position by the neighbouring tissue. I do not say that pockets 5½ mm. in depth are to be regarded as normal. I only wish to justify my unwillingness to draw a sharp line between different depths of pockets, for it is of great importance in practice whether therapeutic action is called for when a pocket is deeper than, say, 2 mm. or not. Taking this point of view I would emphasize that we are not justified in assuming a given depth of a pocket as a reason for doing anything. To sum up, we may say that a pocket without any depth is the ideal. The deeper a pocket the nearer it approaches the pathological, not only on account of the depth itself, but also, and chiefly, on account of the unhygienic conditions of the deep pocket often causing pathological changes.

Have these changes in our idea of a normal pocket any practical bearing? When we extract a tooth of a young person, we often find a zone of white, sound enamel sharply defined against the other enamel which is, as a rule, stained and even here and there partly destroyed. Our predecessors, believing that the bottom of the normal pocket was the amelo-cemental line, explained this white zone as follows: Those parts of the tooth covered by the free gingival margin were protected from all external injury and thus remained healthy, they therefore logically insisted that the gingival margins of all dental restorations—fillings as well as crowns—should be set at those points where no damage can occur. We have, however, found this theory to be incorrect. The sound, white zone of enamel is not a part of the tooth surface in the gingival pocket protected by the gingival flap, but it is a part of the enamel not yet erupted, grown together with the epithelial attachment. It will neither be stained by the action of the fluids of the mouth, nor destroyed by caries. The sharp line to be observed corresponds to the bottom of the pocket and not to the gingival margin. The part protected by the gingival flap is really very small, and, far from being immune, is, locally, exceedingly susceptible to caries owing to the retentive capacity of the pocket. For instance, an area on the buccal side of a molar may become calcified while it is below the gingival margin, but when the tooth is erupted beyond that margin carious destruction often ceases. There remains a dark striation in such a place without further development of caries. To sum up, I would say that the reason for assigning the borders of any dental restoration have been found untenable. On the contrary we have several practical reasons for not doing so in any way possible. I

would like to emphasize that we have good reason to avoid placing the cervical portion of any dental restoration beneath the gingival margin. I have refrained from doing this for some time and am quite satisfied with the results. I hope that those dentists who are now opposed to the use of crowns will not object to the use of crowns which do not reach beneath the gum. Such crowns cannot damage the gum and it is easy to make sure that they fit and that there are no remnants of cementum.

We have now to discuss whether or no the state we have described as existing after clinical eruption has taken place lasts normally throughout life, until the onset of some pathological condition. We may say at once that it is not the case. Just for how long it continues we have been so far unable to discover, but clinical observation and histological study show that eruption continues in the following way: The epithelium becomes detached from the surface of the enamel, and as a rule a downward growth of epithelium along the cementum takes place long before the bottom of the crevice has reached the amelo-cemental line. In the same way atrophy of the crest of the alveolus takes place. The connexion between the crest of the alveolus and the cementum, as well as that between the cementum of the neighbouring teeth, the so-called ligamentum circulare, recedes in a similar manner. In order to illustrate the mode of these occurrences I will show you that the same thing takes place in deciduous teeth, although we know that these teeth do not remain in the jaws for very long.

Before going further I must mention that the epithelial attachment produces a horny cuticle of about 4 microns before it becomes detached from the surface of the tooth: this horny cuticle remains organically connected with the tooth surface when the epithelium is detached. The cuticle is built up in the same way on the surface of the crown as on the surface of the root, and is therefore called the *tooth cuticle*. It is found that this horny mass not only covers the tooth surface but builds up the peripheral portion of the enamel lamella, thus obstructing the entrance to the lamella with a mass of substance insoluble either in acids or in alkali.

We now return to remark on the changes which take place after the eruption of the tooth. A histological examination of a large amount of material, both from human beings and from animals, has revealed the fact that eruption is continuous, though at a varied rate, in individuals. Eruption not only progresses by the operation of raising of the tooth from the alveolus, for at the gingival margin we can observe that the epithelial attachment, as well as the ligamentum circulare and the margin of the alveolus, are moving apically. The lower part of the gingival trough passes beyond the amelo-cemental junction not only in different teeth of the same patient but also round the circumference of the same tooth at different points. We may say that teeth are erupting throughout life and that the slower the rate of eruption the more satisfactory will the denture be. How eruption proceeds is perhaps the most difficult question that can be raised. Some investigators suggest that the slight inflammation to be found in the depth of nearly all gingival troughs causes the downward movement of the epithelial attachment and even the resorption of the alveolar margin, in spite of the fact that the distance between the limit of inflammation and the margin of the alveolus is sometimes very considerable; further, we have also found cases in which the downward growth of the epithelium is seen without any sign of inflammation; this is especially noted in the case of children's teeth. In such instances it has been stated that functional trauma causes a downward movement of the epithelial attachment. We are, however, able to demonstrate that in rats, as well as in rabbits, traumatic deepening of the gingival trough has quite another appearance. There is no doubt that both traumatic influences and inflammation can accelerate the eruption of a tooth, but I think that other factors

largely participate in these changes. The epithelial attachment, as well as the attachment of Sharpey's fibres beneath it, is an organic one. An organic junction between two tissues can only take place and continue when both tissues possess some kind of life. When one part dies, or even when its vitality becomes lowered to a certain degree, then the organic union ceases. Hardly any cell or tissue lives as long as the entire body. Cells and tissues die and are replaced by those newly built up. When the enamel surface dies, a detachment of the epithelium takes place. When the cementum, beginning from the amelo-cemental line, lowers its vitality, then a detachment of the Sharpey fibres takes place and the epithelium grows downwards. While the surface of the cementum has a lowered vitality, the epithelium also becomes detached from the cementum. The formation of the horny cuticle seems also to be a sign of separation of the organic connexion between epithelium and tooth surface. *It is a well-known fact that resorption and deposition take place in the bony socket, while on the cement surface as a rule, with only few exceptions, the deposition of new layers goes on throughout life.* We can often observe the downward growth of the epithelium becoming arrested at a newly deposited layer of cementum. Newly formed cement layers form the best connexion with the Sharpey fibres, and do not allow the epithelium to grow downwards. I believe therefore that the main cause for continuous eruption is a biological one, and that external causes, such as trauma and inflammation, merely accelerate it. That these two last mentioned factors cannot serve to explain everything can also be seen clinically. We often see marginal inflammations lasting for years without the remarkable acceleration of the rate of eruption taking place, the teeth remaining firm. Similar observations regarding trauma can be made. *Summarizing, I would say that it is a fact that the teeth erupt continuously at different rates.* The explanation I have tried to give you is an attempt to demonstrate this. It may be true, but it can also be wrong.

We have already said that one reason for the deepening of a pocket may sometimes be a topographical one; for instance, on the distal side of the last molar. The same may occur at some other place. If in an interproximal space the epithelial attachment on one side is growing downwards rapidly, while on the neighbouring tooth it does not do so, then the papilla cannot follow the epithelial attachment of the first tooth and a deepening of pocket must take place at this point. Another way by which deepening of the pocket may occur is through inflammation of this tissue. Inflammation means hypertrophy. The destruction of the gingival margin means atrophy. Both cannot occur in the same tissues at the same time. In these cases there is downward shifting of the epithelial attachment and no pockets develop. Here pathological conditions can only occur in cases of too rapid eruption of the tooth, so that the part of the root in the jaw becomes in time too short and thus the tooth becomes loosened.

Finally, let us discuss briefly the clinical diagnosis of pyorrhœa. Several years ago I proposed not to make a diagnosis at all in an unclean mouth. It is curious to find that cleansing of the teeth, either from soft or from hard deposit, or the polishing of the gingival margins, or fillings or other similar procedures, is often described as "the therapy of pyorrhœa." It seems to me best to say that we cannot attempt to make a diagnosis when the surfaces are not quite smooth up to the bottom of the pockets. If we do so we can save ourselves the trouble of making a diagnosis in 90 per cent. of the cases. These cases are simply dirty mouths. *In all cases in which in spite of hygienic conditions a discharge continues we shall find without exception that there are deep pockets.* The simplest way to cure pyorrhœa in these cases is to cut away the gingival flap forming the pocket. The main thing in this procedure is not merely to remove the flap but also to prevent renewal of the tissue growth, and this we may do by douching the wound with 8 per cent. zinc chloride. Brushing the gums, according to the methods of Charters, as improved by Stillman, is of the greatest importance.

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[*March* 28, 1927.

Some Aspects of the Innervation of the Teeth.

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(*Lecturer in Anatomy, University of Manchester*).

THIS paper contains an account of an investigation into several features of the innervation of the teeth. It will deal mainly with the morphological and experimental sides of the problem, but will include also some preliminary findings which have been obtained during an inquiry into the sensibility of the teeth.

(1) THE FIBRES OF MUMMERY.

During the last few years several papers on this subject, with descriptions of the literature, have been read before this Section by such able authorities as the late J. Howard Mummery and Professor Hopewell-Smith. It is therefore unnecessary to give a further account here of the work that has already been done. It will be advisable however to consider briefly the opinions of the two above-mentioned investigators. The late Mr. Mummery described a system of non-medullated fibres, which arise from the medullated nerves of the pulp and form the plexus of Raschkow. Immediately below the odontoblasts he found a series of nerve cells, which at their central ends are connected by synapses with fibres of the plexus of Raschkow, while their peripheral ends are drawn out into long fibres which pass through the odontoblast layer to enter the dentinal tubules and end near the dentino-enamel junction. Previously to his finding the nerve cells, Mummery described the fibres which run into the dentine as arising directly from the plexus. Professor Hopewell-Smith was unable to accept Mummery's results and did not consider that the fibres and cells were of nervous origin. He himself holds the view that the nerves end in a basket-like formation around the odontoblasts, without entering the dentine.

This was the position of the problem when the present investigation was commenced. As two eminent authorities held such diametrically opposite views, it appeared advisable for some independent observer to repeat Mummery's experiments in order either to confirm or refute his results.

The members of this Section will recall that in his earlier work Mummery, among other methods, employed Beckwith's gold chloride stain to demonstrate the nerve fibres. Later he devised a modification of that method by which he was able to stain pieces of teeth in bulk, and it was by this method that he was able to detect the nerve-cells. In repeating this author's experiments his earlier technique, by which he discovered the fibres, was carefully followed, and in the first part of this investigation decalcified sections of newly extracted human teeth were stained by the Beckwith method. On examination these preparations were found to have the dentine stained red, and the odontoblasts pink; among the latter were black fibres exactly similar to those described by Mummery.

Fig. 1 illustrates most of the characters of these fibres which their discoverer noted and fully described. It will be seen that some of the fibres are distinctly beaded, a feature to which Mummery attached great importance as being typical of non-myelinated fibres. Others show the markedly wavy appearance which was also observed by that worker. Most of them seem to start immediately internal to the odontoblasts in the region of Raschkow's plexus, which is not seen in this photograph. Some of the fibres can be traced inwards towards the centre of the pulp, but I was never able to establish a connexion between them and the medullated nerves. In this specimen the fibres end suddenly, as if sharply cut off, at the dentinal margin, and the appearance suggests that in a favourable section they might enter

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KEY TO LETTERING OF ILLUSTRATIONS.

D., dentine; *F.M.*, fibres of Mummery; *N.B.*, nerve bundle; *O*, odontoblasts; *R.P.*, plexus of Raschkow.

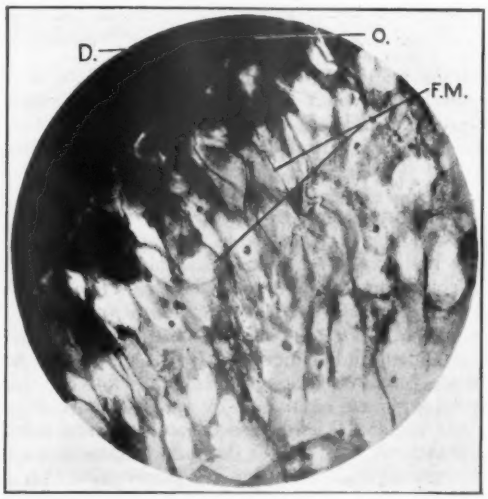

FIG. 1.—Human (× 500). Stain Beckwith.

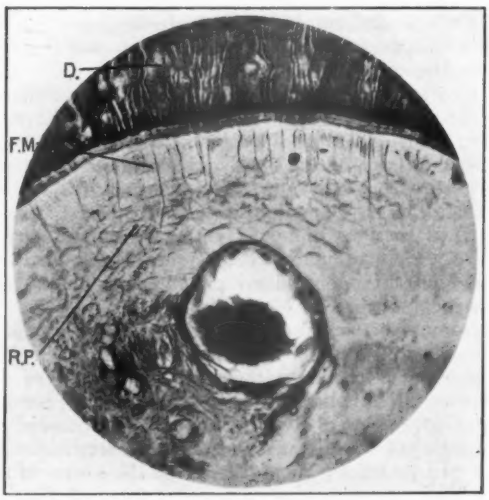

FIG. 2.—Human (× 250). Stain Beckwith.

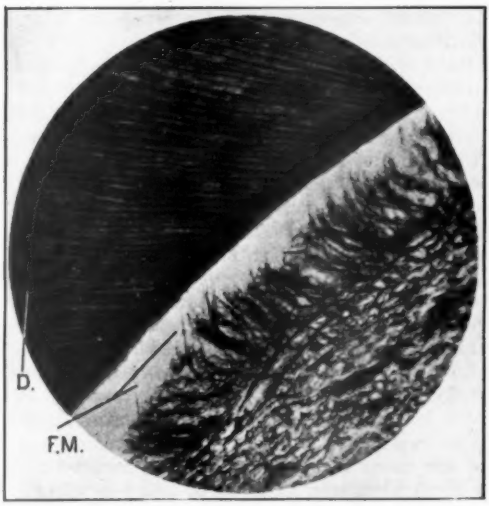

FIG. 3.—Human (× 250). Stain Beckwith.

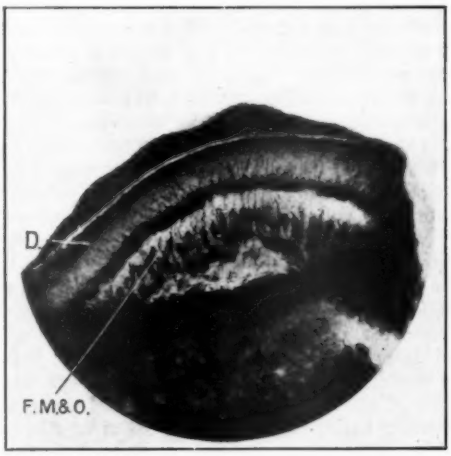

FIG. 4.—Guinea-pig (× 250). Stain Beckwith.

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the dentine. It is not possible to show that they actually do so in this case, but in one specimen they were traced into the dentine. The fibres do not give off side-branches to the odontoblasts, and in some of Mr. Mummery's photographs the same lack of branching will be observed.

In fig. 2 several features of the fibres which have already been mentioned will be seen, and in addition the plexus is also present.

A feature of the fibres of Mummery which that author noted is their elasticity. During the preparation of specimens it frequently happens that a portion of the pulp is torn away from the dentine, leaving a gap which is bridged over by the fibres. These are seen to be under tension, as they are now quite straight instead of wavy, and the beaded appearance is more conspicuous than usual. This condition has been found frequently during the present research, and an example of it is shown in fig. 3.

The Beckwith method was used for staining the teeth of several mammals (monkey, cat, dog, hedgehog, sheep, guinea-pig and rabbit): in every case the fibres of Mummery, showing their characteristic features, were found. An example from the guinea-pig is shown in fig. 4. It will be necessary to return to the subject of the rabbit and guinea-pig's teeth, as their innervation was somewhat peculiar.

In the next portion of this investigation pieces of teeth were stained in bulk with Mummery's modification of Beckwith's method. This turned out to be unsatisfactory. In every case the teeth became black while in the solution, owing to a deposit from the gold forming on the specimen, and on several occasions the solution itself was precipitated. When the teeth were sectioned and examined under a microscope, in only a few were the fibres seen, and in none of them as clearly as in the original Beckwith sections. I communicated with Mr. Mummery, and afterwards had an interview with him, when he informed me that he had had considerable difficulty on account of this precipitation. On my return to Manchester I carried out a series of experiments, with different modifications of the stain, and ultimately obtained a method by which the specimens only turned yellow owing to a slight deposit of the metal. The main alterations in the technique were as follows: The strength of the gold chloride solution was raised from 1 in 5,000 to 1 in 1,000, and then acidulated by the addition of four drops of concentrated hydrochloric acid to every 100 c.c. of solution. An increase was also made in the length of time allowed for reduction. By this means it was possible to obtain sections which, although not free of precipitate, stained the fibres almost as clearly as in the original Beckwith method.

Over a thousand sections were stained by the various forms of the Beckwith method, but in none of them were any structures found which in any way resembled nerve cells. While this part of the work was being carried out Mr. Mummery very kindly sent his section containing these cells to Manchester. It was carefully examined by Professor Stopford and myself and we were compelled to come to the conclusion that there was no morphological evidence that they were of nervous origin. From these observations, and from the fact that in no other part of the body are end neurones found at the terminations of somatic sensory nerves, it must be concluded that there is no evidence that there are any nerve cells in the pulps of the teeth.

This conclusion, however, in no way weakened the claims of Mummery, that the fibres, which he had described, were nervous in nature. From their histological appearance one feels that he was quite justified in coming to that conclusion. It appears to me that he, of all the many workers who have attempted to show that the dentine is innervated, has brought forward the strongest evidence in favour of such a hypothesis.

For the following reasons however it became obvious that some method of approach to the problem other than the histological had to be found:—

(1) The views of Professor Hopewell-Smith that these structures are not nerves.
(2) The fact that metallic impregnation methods are uncertain in their action.
(3) The discovery of certain irregularities in the reactions of these fibres with other stains.

It was therefore decided that an attempt should be made to elucidate the problem by the experimental method. For this purpose it was considered that the study of the effects on the pulp, of section and degeneration of one of the dental nerves would be of great value, and would provide a new means of approach to this question.

The inferior dental nerve was chosen for this purpose, in preference to one of the superior nerves, on account of its being more accessible; and as there is only one nerve to each side of the lower jaw, the risk of overlap was largely avoided. The fact that in man it has been found, from clinical evidences, that the nerve of one side extends over to the canine of the opposite side was borne in mind, and it was thought that fallacies due to this would be avoided by studying teeth posterior to the canine.

The animal selected for these experiments was the cat. It was chosen in preference to the guinea-pig and rabbit on account of certain peculiarities in the nerve supply to the dental pulps of these animals.

In carrying out the section of the nerve the oral route was the one selected. During the operation the anæsthetic was given through a catheter passed into the trachea, which obviated any difficulty in the administration while the operation in the mouth was being performed. In every case the left nerve was divided, and this was reached by an incision about an inch long, the lower part of which passed through the mucous membrane, at the back of the floor of the mouth, just internal to the mandible, while the upper part extended for a short distance upwards, towards the palate. After the incision had been made the first structure encountered was the lingual nerve, which was traced backwards to just behind the anterior border of the internal pterygoid muscle, where it joined the lingual nerve. The latter was now exposed for about $\frac{1}{4}$ inch just before it entered the mandible, separated from its accompanying artery, raised from its bed by means of a hook, and divided with a pair of sharp-pointed scissors. The wound was closed with a continuous suture and painted with collodion. In performing this operation it is essential to have a clear view of the field, and a head light is absolutely essential.

If the nerve is sought for in the way described above there is little hæmorrhage, but if an attempt be made to approach it directly there is a risk of going too far back, when troublesome bleeding will be encountered from branches of the internal maxillary artery, and the risk of sepsis will be greatly increased in consequence.

Three cats were killed two, three and four weeks after operation respectively, and in each case the nerve was found at post-mortem examination to have been successfully divided. This was confirmed by staining the nerves on both sides with Marchi, when the left inferior dental nerves were found to be completely degenerated, while those from the right side were quite normal.

The teeth posterior to the incisors from both sides of the lower jaws of the three animals were fixed and decalcified in the usual way, and cut into sections with a freezing microtome. The sections were then stained by the Beckwith method. They were now examined and, as can be seen in figs. 5 - 8, it was found that in every case the fibres of Mummery were present and equally abundant on both sides. In fact there were no histological points by which the sections of one side could be distinguished from those of the opposite side. The results of these experiments were, I must confess, somewhat unexpected, as the histological appearance of the fibres had left little doubt on my mind that they were of nervous origin.

We will now consider what conclusions are to be drawn from these experiments. They show that Mummery's fibres are not the terminations of the inferior dental nerve. This, however, does not completely exclude the possibility of their being of

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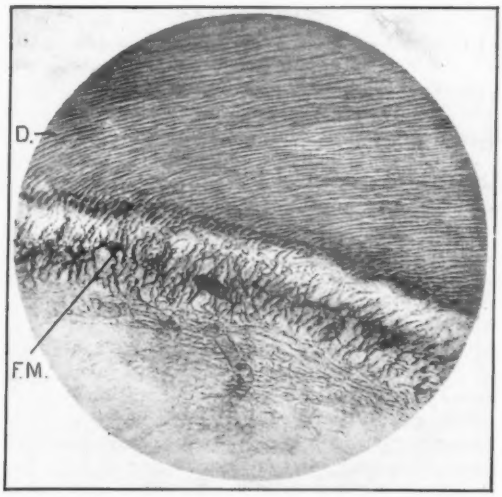

FIG. 5.—Cat, inferior dental nerve, normal (× 250). Stain Beckwith

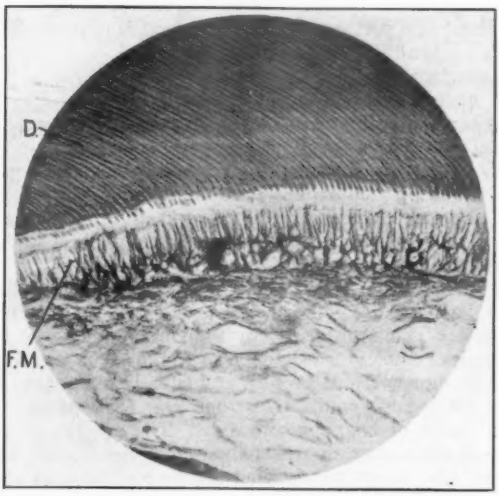

FIG. 6.—Cat, inferior dental nerve degenerate (× 250). Stain Beckwith.

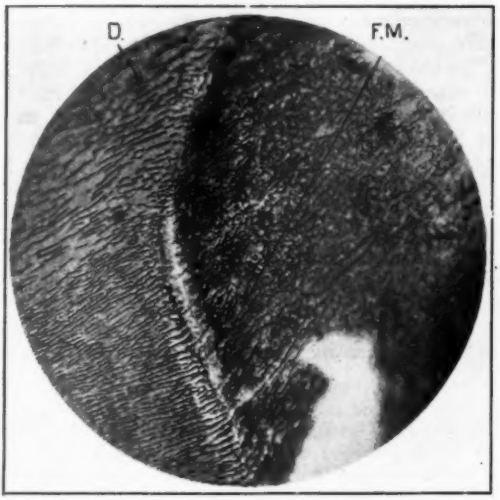

FIG. 7.—Cat, inferior dental nerve, normal (× 250). Stain Beckwith.

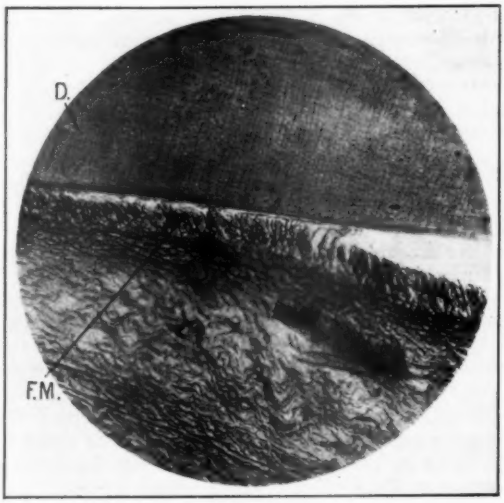

FIG. 8.—Cat, inferior dental nerve, degenerate (× 250). Stain Beckwith.

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nervous origin, as it might be argued that they formed part of a terminal neurone, or came from the peri-arterial sympathetic plexus around the inferior dental artery. In regard to the former argument we have already seen that there is no evidence of the existence of such a neurone, therefore there is no need to discuss this hypothesis in further detail. The question as to whether these fibres may not arise from the sympathetic system cannot at present be definitely settled, with the evidence at our disposal. It is unlikely that the peri-arterial plexus is large enough to supply so many fibres to the different teeth. As it is probable that the plexus receives reinforcements at intervals from the inferior dental nerve, section of that nerve will further diminish the size of the plexus, and thereby make it very difficult to believe that the plexus can be the source of such a large number of fibres. To settle the matter finally, however, it will be necessary to cut both the artery and nerve at the same operation, and it is my intention to do this at an early date.

Attention should be drawn to the fact that these experiments in no way exclude the possibility that the dentine is innervated directly either from the pulp or periodontal membrane. In a later section of this paper certain evidence will be produced which tends to suggest the dentine may receive a nerve supply from the latter source.

(2) The Innervation of the Teeth of Continuous Growth.

In the first portion of this paper it was possible to present results which led to definite conclusions. In the two remaining parts this will not be possible, and the matter contained in them is essentially of a preliminary nature. I am, however, submitting these results for the consideration of the members of this Section, as I hope to receive, especially in regard to the last part of this contribution, suggestions from clinical experience which will be of assistance to me in my future work on these problems.

It is well known that in the pulps of the normal type of mammalian teeth bundles of medullated fibres pass upwards from the root towards the crown, with a course more or less parallel to the long axis of the tooth. Pulps from the teeth of the different animals mentioned earlier in this paper were stained with osmic acid, and cut into serial sections with a view to tracing the arrangement of the bundles. When these were examined a curious fact was noted: it was found that in the rabbit and guinea-pig the pulp-tissue did not contain any medullated fibres. This appeared to be a point of some interest, which, as far as could be gathered from a study of the literature, had not been noted previously. It was therefore decided to examine the specimens more closely, when it was found that although there were minor differences in the relationships of the nerve to the pulp, the general arrangement was fairly constant.

In sections taken from the lower part of the root, nearest to the growing end of the tooth, the nerve bundle or bundles lay in close proximity to the pulp. It will be noticed, however, that in the pulp tissue itself there are no signs of any medullated nerves. As the sections are traced upwards towards the crown the nerve bundle gradually increases in size, and then disappears, and only the pulp tissue remains. The idea suggested from an examination of the serial sections is that the nerves lose their medullary sheaths and then pass into the pulps at different levels as non-medullated nerves. This is, of course, purely a tentative suggestion, as osmic acid does not show up non-medullated fibres. It is hoped, however, to settle this matter by staining specimens with suitable reagents, so as to demonstrate the fibres if they are present.

The final solution of the problem would be of considerable academic importance, as several workers have claimed that they have traced the nerve fibres in these animals, either into the dentine or to the outer margin of the odontoblasts. It would, therefore, be interesting to know the manner in which these fibres, if they are present, enter the pulp.

(3) The Sensibility of the Teeth.

If the literature relating to the innervation of the teeth be studied, it will be found that relatively little attention has been paid to the response of the teeth to the various forms of sensation other than pain.

Among the earlier workers, Hunter [7] and Charles Bell [1] believed that the dentine was insensitive to pain, and that its apparent response to stimuli was due to mechanical impulses which were transmitted to the pulp.

Duval [4] and Thomas Bell [2], some years later, independently brought forward a number of clinical observations which showed that the dentine itself was actually sensitive to pain. This has been the generally accepted view since that time.

Peaslee [9] mentions that the teeth are able to detect various forms of pressure, and that they are able, to a certain extent, to localize such stimuli. Fränkel [5] in 1871 pointed out that this power of localization was due to the nerves of the periodontal membrane, and that it was still present after the removal of the pulp.

Black [3] clearly perceived the difference between the functions of the nerves of the periodontal membrane and those of the pulp. He states that the sense of touch resides wholly in this tissue, which responds to the slightest pressure, while the pulp always gives a painful response to whatever form of stimulus is applied to it.

Woods [10] states that there is probably a considerable amount of tactile sensation in the teeth, over and above the sense of pressure. He adds, however, that a pulpless tooth has much less power of discrimination than an intact one. This implies, although he does not say so, that the pulp contains tactile fibres.

Noyes [8] draws attention to the response of the tooth to pressure and describes the following experiment:—

"Lightly touch the surface of the enamel and the patient will tell at once not only which tooth is touched, but whether a steel or wooden point or some soft material was used. If, however, the finger is placed upon a surface of the tooth and firm pressure made in one direction, the contact of the point will not be recognized."

This phenomenon he explained as being due to slight movements of the teeth stimulating the periodontal membrane.

These are the more important references which have been found on the subject of the sensibility of the teeth. They clearly show that the nerves of the pulp are mainly responsible for the conduction of pain, and those of the periodontal membrane for pressure.

During recent years our knowledge of the functions of the peripheral nerves of the body has been greatly increased by work of Head, Stopford and other investigators. Standard quantitative tests have been evolved by which it is now possible to compare the sensibility of different regions of the surface of the body. These tests also enable us to study the changes which take place when the nerve supply to any area is deranged. At the same time, errors of technique, which are liable to cause fallacies, have been noted and corrected.

It was thought that by applying these methods to the teeth information might be obtained which would throw some light on the innervation of these organs. Pressure-sense was the one chosen for investigation, and in regard to this two points

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were examined: (1) The minimal amount of pressure required to obtain a response in the teeth. (2) The ability of the teeth to localize pressure stimuli.

The experiments were carried out on medical and dental students, and a few of my colleagues on the staff of the Manchester Medical School. The ages of the subjects varied from 18 to 29, except in one case where the age was 40. In the minimal pressure experiments the incisors and canines of both jaws were tested; in each case the centre of the labial surface of the tooth was the point touched, and in the localization experiments the incisors only were used.

It is unnecessary to go into details of the methods used in studying the minimum pressure which the teeth can detect. As far as practicable every precaution was taken to prevent errors arising from faults in technique. One difficulty could not be surmounted. It was impossible at the time to obtain a properly calibrated set of von Frey's hairs. In place of this an æsthesiometer had to be substituted. Head [6] has pointed out that this instrument is not as satisfactory as von Frey's hairs, as it is difficult to calibrate and liable to give varying readings. It was not therefore to be expected that the quantitative results of any great accuracy would be obtained, but it was hoped that qualitative differences of interest would be found.

The quantitative results obtained from 260 teeth varied between 7 and 50 gm./mm.^{2}; three-quarters of these results, or 195, fell between 16 and 32 gm./mm.^{2}: of these 195 teeth, 87 or a third of the total number fell between 17 and 21 gm./mm.^{2} It is believed that these results are too low on account of the roughness of the calibration, but they show that the teeth are at least moderately sensitive to such stimuli.

The qualitative differences noted were slight. Age and sex seemed to make little difference. Subjects examined in the afternoon had a higher minimum than those examined in the morning; this was possibly due to their being slightly more fatigued. The different incisor teeth gave similar results, but the canines were rather higher than the average. Only a few pulpless teeth have been examined, far too few from which to generalize upon, but as far as could be judged there was little difference between them and normal teeth.

In the localization experiments eight different spots were chosen and examined. The results were recorded with the aid of plaster models of the teeth and a chart. As soon as a point is touched its position is noted on the chart, the subject then points out on the model the spot where he thinks the pressure has been applied, and this point is also noted on the chart. This method was adopted to avoid the errors which arise from letting the patient indicate on his own teeth the point touched. In Henri's original method, as quoted by Head, a chart was used. Head found that this was unsatisfactory, on account of the difficulty of depicting a solid object in two dimensions. To get over this difficulty in his experiments on the hand he used the similar hand of another person as an indicator. In the experiments on teeth the objections to a chart apply even more strongly; and plaster models were substituted. They proved quite satisfactory, but certain precautions are necessary. The subjects tested must have some knowledge of the anatomy of the mouth or errors due to ignorance will creep in. There was also a tendency among medical students to confuse the two sides of the model, and to mistake the mid-line of the lower jaw. To overcome the first error, it was pointed out to the subject that such a mistake might occur, and to avoid the second a line was drawn between the central incisors of both models.

On the completion of the experiments, the results for each tooth were plotted on separate charts. By this means it was possible to compare the different points at which various subjects localized pressure stimuli which had been applied to the same

spot in each case. A typical example is shown in fig. 9. It will be noticed that many of the points are closely grouped around the stimulated spot, and that the majority of the subjects have been able to localize with a reasonable amount of success.

The conclusions that can be drawn from these experiments are necessarily tentative. It is obvious, however, that the response of teeth to pressure is fairly acute. The little evidence which we have from pulpless teeth suggests that the nerves of the pulp have nothing to do with pressure, and that this must be transmitted

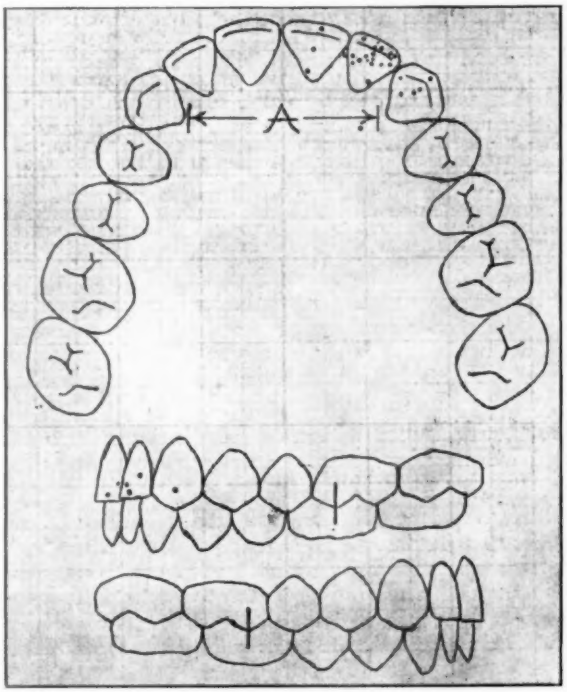

FIG. 9.—Results of points used in localization experiments.

along the nerves of the periodontal membrane. The question arises as to how impulses are conducted from the enamel to the membrane. Can it be due purely to the movement of the teeth, as suggested by Noyes, or is there actual conduction by nerve fibres? The fact that the canines have a higher minimum pressure than the incisors suggests that the former explanation may be correct. On the other hand, the amount of pressure required to obtain a response is so small in some teeth, that it is difficult to believe that this pressure would be sufficient to cause any movement of the teeth. The ability of the teeth to localize also suggests that there must be something more than a purely mechanical movement of the teeth brought into play.

ACKNOWLEDGMENTS.

I have to express my thanks to Professor Stopford for many helpful suggestions during the course of this investigation. To Professor Raper I am indebted for permission to perform the nerve section operations in the Physiology Department of the Manchester University. I am also indebted to Dr. E. W. Fish, and Messrs. Campion and Matthews, who have supplied me with material. Mr. Campion kindly supplied me with my diagrams, from a plate in his possession.

I have to thank Mr. Gooding of the Anatomy Department for the excellent manner in which he has prepared the photomicrographs.

To my wife for her kind assistance in preparing this paper for the press.

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Section of Ophthalmology.

President—Mr. Ernest Clarke, C.V.O., F.R.C.S.

CASES.

Hyaline Bodies (Drusen) on the Optic Disc.

By Ida C. Mann, F.R.C.S.

I thought this was a sufficiently rare condition to justify my showing the case; I have not seen another like it. The patient is a girl, aged 21, who came to the Central London Ophthalmic Hospital with a note from a physician saying that she complained of headache and vomiting, and that the discs appeared blurred. The question of differential diagnosis thus arose. The blurring is seen to be due to the hyaline bodies and the symptoms are migrainous in type. She has recently developed a hæmorrhage on the temporal side of the right disc. I am unable to account for that, except by supposing it is connected with her general toxic state, as she has very septic tonsils. This probably also accounts for her persistent headaches. It has been arranged to have her tonsils removed next week.

? Bilateral Rectus Paralysis or Absence of the Muscle.

By Ida C. Mann, F.R.C.S.

This is a case of apparent paralysis of both external recti. I do not know whether it should be classed as a bilateral external rectus paralysis, or as bilateral congenital absence of the muscle. The patient is an infant aged six months; there is a history of normal labour without instruments, and no subsequent injury has occurred. The mother did not notice a squint until the child was three weeks old. There is said to have been torticollis—head turned to the left—for four months but that has disappeared under massage. Wassermann and von Pirquet reactions negative. I cannot discover anything else wrong with the child. Retinoscopy is + 4 under atropine, and the optic discs appear to be normal, though it is not too easy to see them in so young a child.

Discussion.—Mr. Rayner Batten said he had had a case of congenital double paralysis of the external recti, which he had kept under atropine and watched for a long time. In the course of some years the eyes approached the middle line, and stayed there. The eyes looked peculiar but not disfiguring at the later date.

Dr. A. H. Sinclair (Edinburgh) said that some years ago he had seen a female patient, aged 30, with this condition. Abduction was completely absent in each eye on account of congenital paralysis of both external recti. The patient had perfect single vision when the eyes were directed straight forwards, and she used both eyes together in reading, but when she tried to direct the eyes to right or left, manifest squint appeared on account of complete inability to abduct either eye.

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Mr. HUMPHREY NEAME said he had a similar case which he had been watching for three years. Definite improvement had occurred in the sense that as the child grew it became possible to obtain the movement of abduction in each eye to some extent. Whether that was due to improvement in the muscle, or to improvement in the child's response, he was unable to say.

Mr. AFFLECK GREEVES said he had operated in a case of absence of both internal recti. The eyes of the patient were permanently abducted, and there was no adduction whatever. He had tenotomized both external recti and explored the site of the internal muscles; there was no real muscle present, only fibrous bands, and these he had advanced. Both eyes were now straight, but there was no adduction. The appearance of the child had been thereby much improved, as the eyes were now in the primary position. The age at the time of operation was 1 year.

Mr. ERNEST CLARKE (President) said it was a mistake to speak definitely of this case as one of paralysis. One could not be sure that it was not congenital maldevelopment of the external recti.

Mr. TREACHER COLLINS said he was familiar with cases of congenital defects in the extra-ocular muscles. Mr. Lawford had written a paper on the subject many years ago (*Trans. Ophth. Soc.*, 1888, vii, 262), quoting some Continental observations (D. G. Henck's *Klinische Monats. f. Augenheilk.*, July, 1879) based upon dissections. Instead of muscle fibre, fibrous tissue had developed in some of these cases. He himself had had a case of the kind, in which he had attempted operation to advance the muscle, but he had found only fibrous tissue, of which little could be seized.

Retinitis Punctata Albescens.

By CYRIL WALKER, F.R.C.S.

MISS D. P., aged 33, first seen by me nineteen years ago. I do not think the appearance has altered since I first saw the case. Roughly, the field of vision is normal. I do not think there is any defect such as is usual in retinitis pigmentosa, but possibly careful testing might reveal some colour defect. There is no consanguinity, nor any history of familial defect. Vision for form is not affected, but there is well marked night blindness, which was first noticed when she was about five or six years old, and has not varied. The white dots are all in the deeper layers of the retina; in a very few instances two have coalesced to form a dumb-bell. There are none at the macula, but with that exception the whole of the fundus is studded with them.

I should be glad of suggestions as to investigation of cases with a view to estimating the degree of night blindness. Also as to the degree of improvement likely to follow dark adaptation.

Drawings of Fundus Oculi illustrating Dilatation of Blood-vessels.

By RAYNER BATTEN, M.D.

Mr. RAYNER BATTEN showed a series of fundus pictures depicting dilatation, enlargement, or tortuosity of the blood-vessels of the eye, in both normal and pathological states.

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PAPERS.

Some Facts of Peripheral Vision.

By A. FREELAND FERGUS, M.D., LL.D.

I WISH to call attention to the importance of field vision as distinct from macular. For many years I have confined the term *visual acuteness* to macular vision alone, and states of consciousness due to stimulation of other parts of the retina I, in common with most people, call "field vision." In some of the older text-books the testing of the macular vision is called the testing of the form sense. I demur to any such definition. Form sense is a function which, in health, extends throughout the entire field of vision, although in each eye a certain part of the field is especially differentiated for such work as reading and writing and the seeing of comparatively small objects.

I have formed the opinion, perhaps on insufficient data, that the vital importance of peripheral vision has not been sufficiently considered by most authors. Clearly visual efficiency for certain forms of work cannot be estimated in terms of visual acuteness. I will give some examples as illustrative of this fact. In the first place take a myope say of 10 dioptres; that means that the far point of distinct vision is approximately at 4 in. from the eye. Now a person who has that amount of myopia does not require to be only 4 in. from a tram-car in the street before he sees it. He will, without correction of his myopia, probably see the car at a distance of several hundred feet and with sufficient distinctness to know whether it is the car he wants. Further, he will be able to get on to the car without bringing his eyes down to a distance of only 4 in. from the step. Yet, with the degree of myopia specified, he has no visual acuteness in the strict sense of the term at a greater distance than 4 in. He will probably not be able to read, without correction of his errors of refraction, the board in front of the car indicating its destination. He will, however, be able to determine the colour of the car, and that fact, in a city at all events, will keep him right, apart altogether from visual acuteness. Parenthetically, it may be remarked that the differentiation of cars at night by lights of various colours would possibly be better than differentiation by illuminated numbers.

The second examples are mostly taken from coal-miners. I have recorded cases of coal-miners with high degrees of myopia, in a few cases as high as 20 dioptres—quite efficient, for mining coal, as tested by their pay sheets; they earned quite as much as did the miners with normal sight. Yet these men worked at much the same distance from the coal as the emmetropic or hypermetropic miners. A miner with 4 dioptres of myopia does not require to bring his eyes to 10 in. from the coal, nor does a worker with 20 dioptres of myopia require to bring his eye to 2 in. from the coal. In the one case there can be no visual acuteness, in the true sense of the term, at a greater distance than 10 in., and in the other at a greater distance than 2 in.

The third set of examples refers to seamen. Many years ago I discussed this before a departmental committee of the Board of Trade inquiring into the question of seamen's eyesight.

A recent experiment which I made on myself may be mentioned in this connexion. Standing one night in the village of Arinagour, in the Island of Coll, and wearing a pair of spectacles, which not only corrected my errors of refraction but had in addition a convex spherical element of 3½ dioptres—my ordinary reading glasses—I could see quite well the Treshnish Islands, the nearest of which was approximately 8½ miles from the place where I was standing. I could see them so distinctly that, had I been navigating

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a vessel in these waters, I would have had no difficulty in avoiding them. The night was quite clear, stars visible, but no moon. The observation was made about 10 p.m. on an evening in the month of September. The fact that such observations can be made is essentially due to the light-difference sense, and is not a matter of visual acuteness in the true and legitimate meaning of that phrase. When a person with a fair amount of artificial myopia has no difficulty in seeing such objects, it may well be believed that a person with a natural myopia would see even better, since such a person has, throughout his life, been interpreting images which are not sharply focused on the retina. A person who makes himself myopic only for a short time has not had the same life-long practice in interpreting defective images.

From this point of view there is much to be said for the testing of the light sense of mariners. It is perhaps more important to test the light sense than to test visual acuteness by letters at six metres, or even to test the colour sense.

One great difference between visual acuteness and field vision is that in the first case the correction of errors of refraction is of the greatest importance, in the second it is not; even a considerable amount of uncorrected ametropia makes little difference in field vision. The cardinal functions in field vision are the differentiation of illumination, the perception of movement and the recognition of its absence. Hence the lower animals see thoroughly well for their various purposes, although they may have no macular vision and cannot avail themselves of spectacles!

Little seems to have been done as to the perimetry of the field of visual acuteness. Twice I have made attempts to carry out some experiments on the subject, but I have found that the sources of error were numerous and difficult to eliminate. Probably such investigations would not be of any value either to the physiologist or to the clinician.

It is obvious that in myopia there can be no field of visual acuteness beyond the person's punctum remotum. In emmetropia the field of visual acuteness should, in mydriasis, be of constant size, whilst in hypermetropia it increases, in mydriasis, in area with the distance from the eye, being a function of the tangent of the angle of divergence of the rays as they leave the hypermetropic eye. This increase in the size of the field of macular vision probably explains a phenomenon which I—and, doubtless, other ophthalmic clinicians—have often observed; namely, that a hypermetrope whose vision has been accurately corrected, both for distance and for near, often absolutely declines to use glasses for distance, saying that he sees perfectly well with his own eyes, and is not aware of any defect. Test him with letters and he at once sees the difference made by correction, but for form sense as distinguished from visual acuteness, the correction does not make any perceptible difference to him. The angle of divergence for a given amount of hypermetropia, in complete mydriasis, is a constant; and, therefore, as the distance from the observer increases the vertical or opposite side also increases. In that general increase of the area, the part of the field corresponding to the macular area also increases and comes to occupy a considerable part of the field in distant vision. This circumstance probably accounts for the fact that frequently hypermetropes do not find any benefit from wearing glasses for distance.

I have recorded a number of cases of persons suffering from very defective vision who yet were or had been engaged in various forms of useful work; they are chiefly patients whom I saw two years ago in Glasgow during an investigation which I was making under the Blind Act.

(1) Mr. D., clerk, one-eyed. Amount of myopia in remaining eye is over 20 dioptres, and with this eye he reads Jaeger No. 2. He worked without glasses below ground, as a collier, for seven years in the Bathgate district.

(2) T. Q. Twelve dioptres of myopia in each eye. Worked without glasses in a hangar all through the war.

(3) J. A. Approximately 20 dioptres of myopia in each eye. Was enlisted in the Army (R.S.F.) and served abroad. Since the war worked as a coal trimmer in a steamer.

(4) D. M. Over 20 dioptres of myopia; worked as a fireman.

(5) R. Y. Three dioptres of myopia. Till he became blind from optic nerve atrophy he drove a cart in Glasgow.

(6) J. McE. Operated upon for congenital cataract in boyhood. Has no visual acuteness but has excellent field vision and is able to get about without a guide even in city traffic. When congenital cataract exists to the extent of preventing macular vision, no visual acuteness is likely to develop after operation if that has been delayed till the patient has attained a greater age than six years.

(7) P. D. Left eye has been enucleated. There is an opacity of the right cornea; he admits visual acuteness of $\frac{1}{36}$ and nothing better, yet for many years he has worked as a stair lamplighter and is still doing so.

(This man is probably working by field vision and not by visual acuteness at all.)

(8) J. C., aged 36. Was operated on by myself twenty years ago for congenital cataract. She was then aged 16. She had excellent field vision and has no difficulty in walking about the streets. She can come up from her house in the country, a distance of eight miles, without any assistance, but yet is unable to read. In other words she has excellent field vision but no macular vision. This case, like the one already quoted, shows the danger of delaying operation for congenital cataract unless in those degrees in which the affection is so slight that there is some macular vision through the periphery of the lens.

(9) T. B., aged 53. Left eye completely blind since boyhood. Right eye: 7 dioptres of hypermetropia, yet he worked with this eye, without glasses, till a few years ago as a laundryman in charge of a washing machine.

(10) C. D. Right eye enucleated many years ago. Can count fingers with left at rather less than a metre. With that amount of sight he worked for thirteen and a half years as a stair-lighter in the service of the Glasgow Corporation.

(11) J. F., aged 71. Lost left eye by accident fifty years ago. Worked till September, 1921, as an iron and brass dresser with only one eye. In that year he was paid off and has not been able to get any work since.

(12) W. J. B., aged 30. Severe squint in right eye. The admitted vision of this eye is $\frac{6}{6}$ and it is not improved by any glass. Left eye, spherical + 5, cylinder + 0·75, gives practically normal vision. Patient was in the Army for seven years and nine months and was, in his time, a first-class shot. Is still serving as a Territorial, and as recently as 1925 was rated as a second-class shot.

(13) I. B. Extremely high myopia and slight opacity in each cornea. Vision of better eye $\frac{1}{60}$ and of worse eye $\frac{1}{60}$. For many years employed as a waitress in a tea-room. Out of work at present, but says she is still capable of acting as waitress.

(14) G. W., aged 18. Five dioptres of hypermetropia in each eye with slight astigmatism. Not allowed to wear glasses in the shipyard where he was employed, and in consequence is at present out of work.

The foregoing cases appear to illustrate at least two points: (1) That there are certain forms of work which can be undertaken by persons who have almost no visual acuteness at all but who have good field vision. I have always held that it is impossible to estimate the efficiency of any individual in the labour market in terms of visual acuteness. (2) It must be borne in mind that the legislation which has made industrial insurance compulsory tells very hardly upon people who could work if they were permitted to do it. No doubt that is one of the causes of the increased number of unemployed people. They are unemployable, not from any mental defect, or from any wish to loaf, but simply because recent legislation has made it impossible for insurance companies to run the risk of accidents in their cases. When the Workmen's Compensation Act was first passed, I foretold this in a paper I wrote at that time, and I suggested that there ought to be discoverable some means by which persons handicapped in this way could be employed without the employer running

any risk from the defect—something similar to that scheme which, in ordinary life insurance, embraces what are styled "under-average lives." These persons might perhaps get a little higher rate of remuneration than their more fortunately situated colleagues—that slight extra remuneration should relieve the employer altogether from liability in their case for any accident that might occur owing to their defects.

Mr. Percival Pott on the Treatment of Lachrymal Fistula.

By A. Freeland Fergus, M.D., LL.D.

The name of Percival Pott, an eminent London surgeon of the middle of the eighteenth century, is honourably associated with two conditions to which he devoted special attention. Pott's curvature and Pott's fracture are still household words in surgical nomenclature. But it does not seem to be generally known that he published an interesting monograph on diseases of the lachrymal sac, in the year 1758, the full title of which is "Observations on that Disorder of the Corner of the Eye commonly called Fistula Lachrymalis, by Percival Pott, Surgeon to St. Bartholomew's Hospital, London. Printed for C. Hitch and L. Hawes, in Pater Noster Row, 1758." I happen to possess a copy of the book, and no doubt a short account of it may interest Members of the Section.

In those days a considerable number of practitioners from the provinces seem to have frequented London, as they still do, and as they probably always will, for post-graduate study. In his introduction, Pott says he found that they had devoted little attention to what appeared to them to be an intractable disease—at any rate that was his opinion after comparing notes with the gentlemen who seem to have attended his instruction. At the beginning of his work, he informs us that the generally held opinion before his time was that the lachrymal fluid was secreted by the caruncle. The function of the lachrymal gland was at that time not understood; it was therefore called the innominate gland, and the swelling at the inner canthus, which we now know to be an abscess of the lachrymal sac, was in old days supposed to be due to an abscess or inflammation of the caruncle. Percival Pott was well aware of the true nature of things. He says:—

> "In a healthy state the fluid secreted by the lachrymal gland and membranes of the eyelids passes all through the puncta, sacculus and duct into the nose without any trouble, but in a diseased state, when the membranes are inflamed or thickened, the nasal duct becomes obstructed, whereby the course of this fluid is either much impeded or totally stopped, in consequence of which the natural mucus of the sacculus fills it and prevents it from receiving the lymph from the lachrymal gland; it therefore runs off the eyelid down the cheek; the obstruction continuing and the mucus still lodging the sacculus becomes dilated, and produces that tumour in the caruncle of the eye, and that discharge upon pressure which characterise the disease called fistula lachrymalis; and in conjunction with every other attending symptom which proves its seat to be in the lachrymal sac and nasal duct."

I am not sufficiently well versed in such matters as to be able to say whether Percival Pott was the first to state clearly what was the passage of the tears from the lachrymal gland to the nares, but certainly he fully realized that the caruncle had nothing to do with the secretion of tears. The treatment preceding Pott's time was incision of the supposed abscess of the caruncle and, when the contents were purulent, the application of caustics and cautery to the depth of the cavity, under the belief that there was caries of the bone. He goes on to narrate:—

"The present practitioners, finding that an obstruction of the lachrymal sac and duct is the true origin of the disorder and that an abscess in these parts is most frequently a consequence, and not a cause, have with great industry and ingenuity endeavoured to find out some means whereby this obstruction may be removed, and the parts restored to their natural and healthy state without such pain, destruction and deformity as the antient (*sic*) methods produced."

Very early in his short treatise, now under consideration, the author gives a description of the anatomy of the parts and of the passage of the tears across the eye, their absorption by the puncta lachrymalia, and their drainage down the sac into the nasal duct and thence into the nares. He mentions that the sac and nasal duct are covered by a pituitary membrane similar to that which covers the inside of the nose.[1]

Following upon this we find a description of the difference between mucus and pus. It is odd to find that, as the result of his investigations on the subject, Pott, who in his day was one of the most prominent surgeons in England and deservedly held in high esteem, came to the conclusion that the discharge from the urethra in acute gonorrhœa is not pus, and that there never is an ulcer or abscess within the lachrymal sac.

At the end of Section III, the one from which I have just been making extracts, he has some remarks on the ætiology of the disease; these I quote *in extenso*:—

"If the cellular membrane inflames the redness is of greater or lesser extent in different cases; in some it is confined merely to the surface of the sacculus in the corner of the eye, in others it spreads still farther and affects the eye lids, cheek, or side of the nose."

"If the puncta lachrymalia are naturally large, the contents of the sacculus will pass off so freely, that tho' the inflammation is considerable, the sacculus a good deal dilated, and the discharge apparently purulent, yet the skin will remain entire; while it does so, the disease is by the antient writers called simple, imperfect, or anchylops."

"But it often happens, either from the puncta being too small to let the matter pass off freely, or from the cellular membrane inflaming and becoming sloughy, that the skin covering the sacculus bursts, and an opening is made externally in the angle of the eye; when this happens, the disease is said to be perfect, and is called aigylops or aegylops."

"In this state the discharge which used to be made through the puncta lachrymalia is made principally through the new opening in the skin, and by excoriating the eye lids and cheek, increases the inflammation; in some the matter bursts through a small hole, and after it has discharged itself, the tumour subsides, and the parts become cool; in others the breach is large and the sore foul and sloughy, the skin remains hard and inflamed, as well as the caruncle and eyelid, and the discharge is large; sometimes when the case has been neglected or ill-treated, a loose fungus occupies the cavity of the sacculus, and sometimes the bone underneath is found carious."

"This last circumstance was by the antients supposed to happen very often; but since its frequency has been doubted, and the case has been more minutely inquired into, it has seldom been met with, and may be regarded as a rare thing, unless the habit of the patient is infected by the lues venerea, or the sac has been the seat of a variolous abscess."

"These are the general appearances of this disease, when considered by itself; but it very often happens that it is combined with other diseases both local and general, by which the prognostic as well as the method of treatment must be varied; for instance, it is often connected with an habitual ophthalmy or lippitudo; sometimes with an ozena or some disease of the membrane and cells of the ethmoid bone; sometimes it is produced by the pressure of a polypose excrescence in the cavity of the nose; the habit in some is infected with lues venerea, of which this may be a symptom; strumous glandular obstructions are its too frequent companions; and what is worst of all, it is sometimes cancerous."

In beginning the discussion of treatment, Pott asserts that the characteristic of the disease is a lodgement in the sac of a quantity of mucus mixed with the fluid

1 The word pituitary is of course derived from *pituita*, which means phlegm.

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from the lachrymal gland; and that the original cause of this lodgement is an obstruction of the natural passage from the sac into the nose. The removal of that obstruction is the first curative intention.

To facilitate the discussion of treatment, Pott divided all cases of what we call dacryocystitis into three groups, in the following manner:—

(1) "Simple dilatation of the sacculus and obstruction of the nasal duct, without any inflammation, and the discharge (upon pressure) of a mucus either quite clear or a little cloudy."

(2) "Inflammation, abscess, or ulceration of the same parts, with the discharge of a purulent mucus, or of matter." (By "matter" the author means pus.)

(3) "Obliteration or destruction of the natural duct, attended sometimes with caries of the bone."

In connexion with the treatment of the first group of cases, Pott mentions the use of lachrymal probes and of lachrymal syringing only to condemn probing and to give a very guarded approval of the syringe. The probe which he used, but of which he did not approve, and the lachrymal syringe, he informs the reader, were the invention of Anel. Anel's name is still sometimes associated with lachrymal syringes.

Another method of treatment in common use was the application of external pressure on the sac, which was sometimes applied by instruments specially constructed for the purpose, or by the application of an ordinary compress. While not condemning such applications altogether, Pott points out that their use may produce inflammation and do harm, for "any defluxion of an inflammatory kind will infallibly add to the obstruction of the nasal duct."

In speaking of the second stage, viz., "that in which the parts are inflamed or ulcerated," Pott laid special stress on early incision:—

"Without opening the sac there can be no remedy. An incision should be made before there is any breach of the skin by sloughing and bursting of the sac, for the scar resulting from a wound made by a knife will be less disagreeable than one made by bursting. If incision followed by simple dressings does not cure the disease, it becomes necessary to try other means; always keeping in view that the end to be obtained is to render the nasal duct pervious to the lachrymal fluid."

Notwithstanding the contempt in which he held Anel's probe, Pott indicates that the best way of obtaining the patency of the sac and duct is by passing, at each dressing, a probe either of silver or of whalebone, a piece of cat-gut, a plaster bougie, or anything of the like sort.

As was to be expected, Pott discusses at some length the treatment of lachrymal sac inflammation by the use of severe escharotics inserted into the cavity after the incision is made. This he condemns for the reason that he does not want pus from the cavity itself but only from the lips of the wound.

It will be understood, of course, that setons were sometimes used by Pott. He borrowed this practice from Dr. Monro, of Edinburgh, for he says:—

"Dr. Monro, of Edinburgh, in a very ingenious paper on this subject, published in 'Medical Essays,' advises to make a passage with an awl thro' the fundus, and afterward

by means of a probe to draw a seton into the nose; which seton may be armed with such medicines as may serve either to repress the fungus, or establish and heal the passage."

Pott goes on to remark:—

"I cannot say that I have ever found it necessary to use the awl, but have reaped much advantage from the passage of the seton where it has been practicable; which from the different disposition and size of the ossa spongiosa is sometimes easy to execute and sometimes impossible."

In Section V, which is the last section of this short monograph, Percival Pott discusses the third stage of this disease, in which stage "the natural passage is quite obliterated and destroyed, and in which the bone is sometimes found carious." When obliteration has taken place, then the question arises as to the making of an artificial opening.

The method which the author adopted was the perforation of the os unguis. The os unguis is of course the lachrymal bone.

He says that if this be done there is at once a communication between the lachrymal sac and the nose. Pott states quite clearly that this operation was no invention of the moderns, and that the "antients" perforated the os unguis both with a cautery and with a terebra. He further admits that:—

"The operation was executed much in the same manner as it now is, yet I think it very clear it was not done by the same intention."

The os unguis was sometimes perforated by the cautery and sometimes by a terebra. Our author, however, is of the opinion that the "antients" did not intend the formation of a new passage, for they were entirely ignorant of the fact that the tears passed from the conjunctival sac into the nares by the sac and nasal duct. He says:—

"Destruction of the callosity and exfoliation of the caries were all they had in view, and the perforation of the os unguis was either accidental or made merely for a temporary discharge of matter."

Pott regarded an opening through the unguis as being essential when the natural passage is not available, and indicates that such a procedure was in common practice in his time; some surgeons effecting the perforation by means of the cautery and others using instruments like a terebra to break through the bone. Our author entirely condemns the cautery. He briefly sums up the object of treatment in these words:—

"The intention is to make an opening through this bone and membrana narium into the cavity of the nose, and to treat that opening in such a manner as that it shall most probably remain open, and give passage to the lachrymal fluid from the puncta lachrymalia, after the sore is healed."

He mentions certain instruments as being well adapted for the making of the perforation; he says that any of them will be quite suitable for the purpose; but whichever is selected, the essential point is to perforate the unguis immediately behind the sac.

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Of recent years several operations have been introduced into practice, notably by Dr. Polyak, of Budapest, and by Dr. J. M. West. These modern operations differ entirely from that of Percival Pott, in so far as they are intranasal and are more complicated. For many years I have thought that the chief point to be attended to in dealing with suppuration of the lachrymal sac is the thorough and efficient removal of a septic membrane. If the lachrymal sac has once become septic it is very difficult to get rid of the sepsis. I have no objection however to the attempt being tried by careful syringing. When a surgeon has made up his mind to remove the sac, and does so, the operation should always be accompanied by the removal of the lachrymal gland. It has long been my opinion that the lachrymal gland only responds to special stimuli, very much in the same way as the testicle does, although of course to a different class of stimuli. The ordinary lubrication of the conjunctiva and of the cornea seems to me to depend upon the numerous glands which are to be found in the upper part of the conjunctiva lining the superior eyelid.

Section of Orthopædics.

President—Dr. W. T. Gordon Pugh.

Two Cases of Spastic Paralysis Associated with Scoliosis.

By R. C. Elmslie, O.B.E., M.S.

At a meeting of this Section last year I showed two cases of scoliosis with spastic paraplegia; I have since had four others, two of which I present to-day. This condition has always been considered to be extremely rare, and it is interesting to note that Pott differentiated spinal caries from lateral curvature of the spine on the ground that the former might be associated with paraplegia, whereas in lateral curvature paraplegia never occurred. In the two cases shown last year I confirmed the site of obstruction in the spinal theca by lipiodol injections and performed laminectomy; in the present two cases I have neither injected lipiodol nor operated.

Case I.—Patient, a boy, aged 16; the scoliosis is said to have commenced four years ago; the onset was gradual, and the disease has progressed steadily. For six months he has had difficulty in walking. He has a very severe left dorsal scoliosis with a large kyphotic curve. His gait is spastic, and his reflexes are altered, in that the lower abdominal reflex is lost; the knee-jerks are very much increased; there is a patellar clonus and an extensor plantar response. There is no loss of tactile sensation. He has been suspended on a Fisher bed for three months, and his spasticity is now less.

Case II.—Boy, aged 17, first attended St. Bartholomew's Hospital when aged 10 on account of a structural scoliosis with bony changes, though not very severe. After attending the hospital for four years he disappeared and was lost sight of in 1922. On March 10 last he returned, exhibiting a very severe right dorsal and left lumbar scoliosis with much rotation, much kyphosis, spastic paraplegia, loss of both abdominal reflexes, increased knee-jerks, patellar clonus, ankle clonus and an extensor plantar response. He has not been taken into hospital for treatment, but I shall treat him by suspension on the Fisher frame in the same way as I have treated the others. It is as yet too early to say much about the result of this suspension. The first patient I treated in this way, from being completely paralysed with complete anæsthesia to the level of the fifth dorsal vertebra, has lost his anæsthesia and has recovered sufficient control to walk about, though he still has a spastic gait. He still suspends himself at home during four hours of the day.

Discussion.—Dr. W. T. Gordon Pugh (President) asked Mr. Elmslie to state a little more detail as to the treatment he adopted; and also to say whether he found that treatment useful in cases of paraplegia due to spinal caries.

Mr. ELMSLIE (in reply to the President's question) said that the method of suspension was by a Sayre's sling fixed on to the back of an adjustable bed-rest, which was raised until it was nearly vertical. A rack and pinion behind the bed-rest enabled the sling to be raised or lowered. Sometimes only chin and occipital straps were used; sometimes arm straps. The patient was lifted until the buttocks were just clear of the bed. The period of suspension depended upon the weight of the patient. One of the boys was being suspended every alternate half hour throughout the day.

As to the pathological anatomy, in the first case on which a laminectomy was performed the spinal cord was found tightly tucked into the corner on the concave side of the vertebral bodies, so that the laminæ on this side had to be extensively removed before the theca could be seen. No cerebro-spinal fluid escaped when the theca was incised until the opening had been carried well above the most severe point of the curvature. The theca was so tightly stretched that it could not be sutured. The interference with the function of the cord appeared to be due rather to this acute tension around the curve than to any particular point of pressure.

A Method of Treating Lumbar Scoliosis.

By C. LAMBRINUDI, F.R.C.S.

THIS is an attempt to devise a method of treatment for lumbar and dorso-lumbar scoliosis.

Case I.—Patient, a girl, came up two years ago with a deviation of the spine to the left and a flat back. She was given the usual exercises, but in the course of time, in spite of the treatment, she developed a marked curve in the left lumbar region with rotation and early fixation of the vertebral bodies and complete flattening in the lumbar region.

I put her up in a short plaster spica with the right leg flexed and abducted carrying the plaster above the left iliac crest. Consequently, when she brings her legs together in the normal standing position, she automatically pulls the lumbar spine over to the right and, at the same time, is forced to arch her back.

This appears to me to be a reasonable method, because it imposes the attitude of activity while active, permits of the attitude of rest, and allows the remedial exercises to be carried out more advantageously, since they can be performed when the patient is enforcedly standing in the corrected attitude.

You only have my word for it, but I assure you her state was markedly improved after wearing the spica for only three weeks.

Case II.—Patient, a girl, whose condition illustrates a class of case which I believe is liable to be made worse by the usual exercises for a weak back.

She has a long, narrow, generalized, kyphotic back, and stands in an almost exaggerated attitude of rest. She cannot touch her toes by about 8 in., but when she stoops to her utmost limit, so far as her back is concerned it is normal, both in range of flexion and shape (figs. 1, 2). She fails to touch her toes because her hamstrings are remarkably short. If she were made forcibly to stoop down and touch her toes, as children are apt to do in the ordinary gymnastic classes, undue pressure would be brought to bear upon the bodies of her lumbar vertebræ. She would be trying to flex her spine beyond its normal limits; consequently the vertebral bodies would be liable to react in one of two ways: either they would become wedge-shaped or they would rotate out of the way.

I have seen cases such as this gradually progress from merely an abnormal attitude to a fixed lumbar kyphosis or scoliosis with rotation, in spite of treatment. I maintain that this has probably occurred because of the treatment.

In her case I propose to concentrate on passive stretching of the hamstrings when she is lying flat on her back. If they are too resistant I shall not hesitate openly to lengthen their tendons. When they are sufficiently lengthened I shall put her up in a double spica with the hips flexed; in other words I shall, as it were, temporarily ankylose her hips in a suitable position.

In order to stand erect with her hips thus fixed, she will be forced by her own effort to arch her back and so to correct her attitude. When I have done what I suggest, I hope to show her to you again.

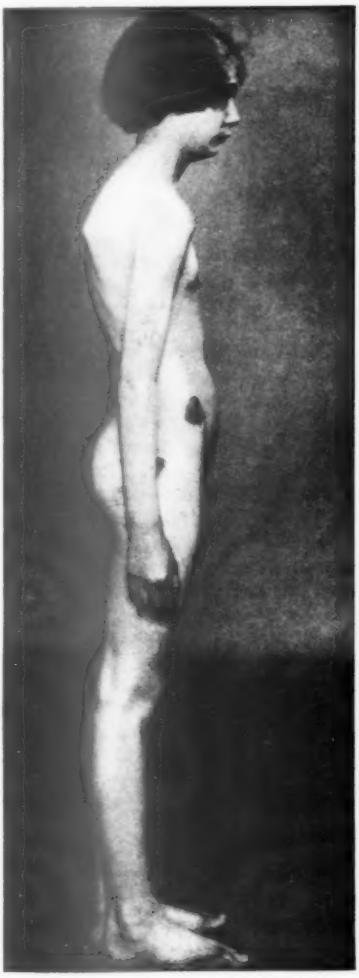

FIG. 1.

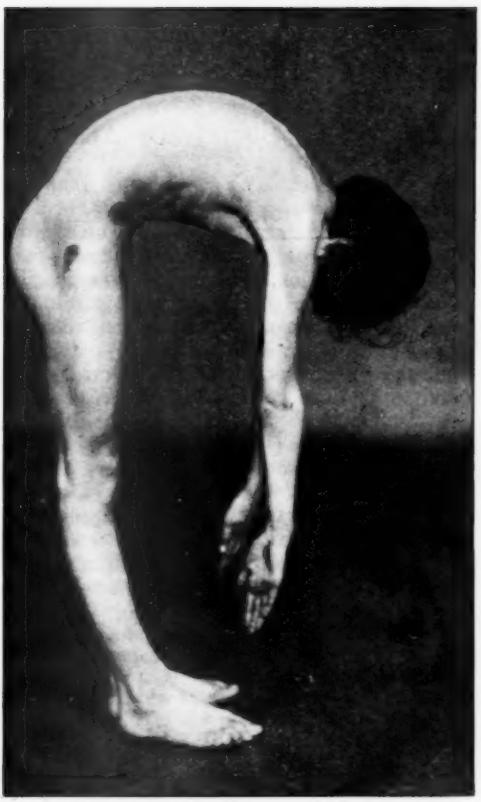

FIG. 2.

60 Lambrinudi: *Lumbar Scoliosis*; Todd: *? Periostitis of Metatarsal*

Discussion.—Dr. GORDON PUGH (President) said that children with a hip ankylosed in adduction with only slight flexion were apt to develop structural scoliosis, particularly the older girls. If the hip was fixed in a position of adduction with a flexion of 40° or more, however, this did not occur, for it was then not necessary for the child to bend the spine over towards the affected side on sitting, although in standing or lying it was curved laterally through tilting of the pelvis. But if the adduction was associated with only slight flexion, it was necessary for the child to bend the spine over to the affected side in sitting, and thus, whether she were lying, standing or sitting, the pelvis was tilted up on the affected side and at no time was the spine at right angles with the interspinous line. The child almost always developed structural scoliosis with the concavity towards the affected hip. If he (the speaker) was correct in assuming that Mr. Lambrinudi was endeavouring to undo a lumbar scoliosis by over-extending and adducting the hip on the side of the *convexity*, he thought the method might prove useful, and he regarded the suggestion as a very interesting one.

Mr. LAMBRINUDI (in reply) said that in the first case he did not try to fix the pelvis; what he made use of was a leverage action, the plaster spica came above the iliac crest, so that when the patient adducted her leg she automatically pulled her lumbar spine over to the right. It was a direct pull. In the second case, however, he did propose to fix the pelvis with the hips flexed, as Dr. Pugh stated. By so doing he hoped that the patient would develop a compensatory lordosis, and thus, by her own effort, correct her attitude.

Case of ? Periostitis of Metatarsal.

By ALAN TODD, M.S.

THIS woman, aged 32, told me that there had been no injury of her foot. She complained first in November, 1925, of a red, painful swelling at the neck of the second left metatarsal. There was an obvious firm, rounded swelling there. She was hardly able to walk at all, and she had a painful limp. She has never fully recovered from the condition. She complains a good deal, and says that she cannot walk more than a quarter of a mile. Still, the pain is much less acute than it was. The skiagram originally taken cannot be found, but there was no evidence of fracture, not even of a crack; there was a little "fuzzy" patch of what I think was typical periostitis on one side of the metatarsal, and gradually that has become better defined, until it has arrived at what you see to-day. We have investigated the case in every way. The dentist, laryngologist and radiologist give her a clean bill of health; her Widal and her Wassermann reactions are both negative.

I do not know what is the cause of her condition. The patient and I are most concerned to know how to relieve it, as she is a good deal crippled by it. She has bunions and splay feet, but her symptoms have always been referred to the site of that bony thickening. Perhaps it would be worth while to channel that patch of sclerosed bone; I do not think drilling it would do any permanent good. Strapping the foot over a Scott's dressing has relieved her, temporarily, to some extent.

Discussion.—Mr. B. WHITCHURCH HOWELL said he thought this was a case of "marching fracture" of the second metatarsal bone in a flat foot which had bunions. Perhaps it was a subperiosteal fracture. Walking in the pronated position on the inside of the foot had caused fibular deviation of the distal end, with a buttressing of thick periosteal bone on that side. This, in his view, was the whole cause of the trouble, and he thought the treatment to be adopted should be that of flat foot.

Mr. W. R. BRISTOW said he differed from both the exhibitor and Mr. Howell as to the treatment. In the examination room this woman told him definitely that her pain was about the metatarsal head. Looking at the skiagram one could see an osteophyte extending between

the first and second metatarsals, and if she were under his care he would excise the head of the bone, restore the anterior arch, and let the bunion take care of itself. He did not see why the thickened second metatarsal bone should cause pain; it was not tender on direct palpation, and he doubted whether the patient would be relieved, either by boring or by channelling the bone.

Mr. T. H. OPENSHAW thought there must be a crack in the bone, i.e., in the second metatarsal. He did not see any osteophyte on the outer side of the head of the first metatarsal bone: he thought the round lump seen in the X-ray was a sesamoid bone. The exostosis on the inner side of the head of the first metatarsal bone was about the usual amount of enlargement. He did not believe in removing the head of the bone; he had done this operation on a patient recently, and her toe was now much more stiff than was any great toe he had operated upon for years. He had operated upon most great toes for hallux valgus by shaving off the exostosis and there was never any resulting stiffness of the metatarso-phalangeal joint. Perhaps the exostosis in this case should be shaved off. He did not see any reason why this patient should get excessive pain as the result of her periostitis on the inner side of the shaft of the second metatarsal.

Mr. ALAN TODD (in reply) said it was unfortunate that he could not show the original radiograms. If there was a crack, it was not detectable. There was at first a fuzzy concave-surfaced patch, like a periostitis due to focal sepsis elsewhere, such as the teeth, and only gradually, in the course of two years, did it assume the well-defined appearance now seen. From the first the patient had referred her pain consistently and exclusively to the tender lump; he did not know whether there was now a neurotic element about it, but in the first place probably the pain was definitely located there. Someone had pointed out that the inflammatory condition was not confined solely to that thickening at the neck of the metatarsal; that there was much thickening right down the shaft of the bone. One could clearly see in the negative a little barrel-shaped thickening of the shafts of the phalanges, confirmatory evidence, he thought, of a slight septic condition in the foot. These findings were perhaps in favour of his view that this was a septic periostitis rather than an inflammatory response to an injury.

Bilateral Contracture of Sterno-mastoid Muscles.

By S. L. HIGGS, F.R.C.S.

PATIENT, a girl, aged 9 years, the youngest of four in the family, the other children being normal. The condition, I think, is a very rare one. The mother says that the birth of this child was uncomplicated, but she thinks forceps were used. It was a vertex presentation. At birth the child weighed only 3½ lb. The child has had operations for removal of tonsils and adenoids, one at two years of age, the other at six years. The mother noted that in infancy the child had a short neck, and that recently she has been becoming increasingly round-shouldered. At present there is contracture of both sterno-mastoids, more marked on the left side than on the right; there is also slight asymmetry of the face, fixed kyphosis, and marked prominence of the upper part of the sternum and the anterior ends of the ribs. The inner ends of the clavicles are high, and the neck is short. Movements are fairly free, except extension, which is limited. Skiagrams show no abnormality in the spine.

I propose to do open division of both sterno-mastoids, and follow that up by stretching and exercises, and making her rest on a plaster bed with the head-piece well dropped back so as to maintain extension of the head.

Have other Members had experience of this bilateral condition, and can they give me any suggestions as to treatment? I do not want now to bring up the vexed

62 Higgs: *Bilateral Contracture of Sterno-mastoid Muscles*

question of the pathology of sterno-mastoid torticollis; but the fact that the bilateral condition is so rarely met with must have some significance.

Mr. WHITCHURCH HOWELL said that six months ago he had had a similar case in a girl a little older than this patient, but the contraction in that case was not so marked as here. X-ray films showed nothing. In these cases one should be on the look-out for a Sprengel's shoulder, in which condition one often found wedged vertebræ and scoliosis, and, perhaps, a cervical rib. In the skiagram there was a feature of the seventh cervical which might suggest a cervical rib.

The treatment suggested by Mr. Higgs would be of value. In putting these patients into plaster of Paris it was very difficult to get the head back, and it might be well to have the throat of this patient examined beforehand. Probably the reason why the tonsils had been operated upon twice was on account of the difficulty of enucleating them, as the jaw could not be opened sufficiently for good access to the tonsillar region.

War Section.

President—Group Captain HENRY COOPER, D.S.O., R.A.F.

Man-power and the Medical Service in Relation to some of the Principles of War.

By Major T. J. MITCHELL, D.S.O., M.D., R.A.M.C., D.A.D.G., War Office.

IN discussing "Man-power and the Medical Service in Relation to some of the Principles of War"—e.g., concentration of superior force both moral and material, economy of force, mobility, and co-operation,—I wish to state most emphatically that I believe the medical service exists to assist the army in the field, and that all sections of our Corps are necessary for the well-being of that army. I feel that when our President coined the phrase "a groove and how to get out out of it," he was trying to get us away from the present-day habit of taking things casually as we find them. I believe such a call is necessary, and I shall endeavour to follow it to-night.

Our official text-books stress the usual functions of the medical service—preservation of the health of the troops, treatment and care of the sick and wounded, replenishment of medical and surgical equipment and the collection and evacuation of sick and wounded. But what of the wider issues? We seldom refer to economic facts and our ability to save the State both men and money. The Medical Services have, so far, offered no contribution to this important subject, probably because we have been concentrating on our tactical and technical problems. I should like to direct your thoughts towards a wider conception of the Medical Service in its relation to the Army, the State and humanity.

Man-power and attrition were important factors in the Great War, and they are likely to become even more important in future wars. The French realize this, for it was stated in the French Chamber that another war "would not be merely a military war, but would involve the whole nation, throwing all classes and both sexes into the fight, and absorbing the entire resources of the nation." It is right, therefore, that we should reconsider our position and contribute to the common stock the results of our unique experience during the Great War.

Our functions as a medical service are like those of a commercial company in which those asked to take shares are given no information as to the dividends, and I feel that if I were an Army commander in the field and had to support an efficient medical service, my first question to my Director-General would be: "Will you inform me how many men or what percentage of casualties you will save and return to active duty?" Some of you who entered the Service, determined to retain hold of some special professional subject throughout your career, might be able to tell the number treated for a particular disease and made fit for duty in a short time. Others, like myself, who were willing to take whatever the goddess of chance cast in their way, might have more difficulty. Personally, I have no complaint against the goddess, but to-night I have certain qualms because I am parading for the first time a horse bred by a famous statistical stable, the "Medical Research Council," almost warned off the course by the Finance Department at the War Office and which has never appeared in public before. Our late revered chief, Sir William Leishman, persuaded me to train this dark horse, and the first time I mounted him I was thrown into a gloomy and uncharted forest, known as the Medical Statistics of the Great War. Fortunately,

my national caution enabled me to ensure my official life by retaining the right to nominate my own colleague, and it is owing to the loyal co-operation of my colleague, Miss Smith, that I am able to present to you some interesting new facts.

In presenting these figures let me say definitely that necessarily they are all approximate, but they are sufficiently accurate for the lessons I wish to draw from them. I specially ask you to note the following points:—

(1) The large numbers of men constituting the different forces.
(2) The different countries and climates in which we operated.
(3) The number of followers attached to some of the forces.

(1) Numbers.

With this table as a basis the broad questions of recruiting, training, supply of troops fit for Eastern theatres of war, billeting, rations, clothing, transport and the provision of hospitals, should be considered from the point of view of possible future wars. As Field-Marshal Sir William Robertson aptly said: "The longer peace continues the more difficult it is to prepare for war."[1]

The medical services are now guided by men who won their spurs on the field of battle. They are young, active and experienced in dealing with the casualties and sick among large bodies of men, and they will enhance the glorious reputation we had earned at the end of the last war because their memories are still fresh. But what will happen in ten or fifteen years? We have entered upon a régime of financial stringency; a service nurtured on that policy is starved, and starvation leads to anæmia and inertia. This is exactly what happened in 1914: witness our opening disasters in almost every theatre of war, especially in Mesopotamia. I remember well my first experience of that country. I had come from France with broadened views; the conditions were brought home to me most forcibly when I saw a man who had lain for ten days on a river boat with a compound fracture of the thigh. This was at the base. Further up the line, few or no medical arrangements were made, or could be made, for there were neither men nor material: it was largely due to this that the *morale* of the British troops was low, and without *morale* among the troops no commander in the field can make good. The rot was stayed by the arrival of the medical units of the Indian Corps from France, whose endurance and organization had been stimulated by hard experience on the Western Front. Later the position was consolidated and reorganized into one of the finest shows we had. What happened in Mesopotamia will happen again, if in your spare hours you are content to think in battalions, brigades and divisions, and do not dream of and work out the broader medical requirements of a nation in arms.

(2) Countries.

My second point is to draw your attention to the varying climatic and physical features of the countries in which the Great War was fought. It is well known that a certain standard of equipment and personnel is laid down for medical units on mobilization, and that this proved suitable for European theatres of war. But the transport was absolutely out of place in the mountains of Macedonia, the roadless peninsula of Gallipoli, the bush of East Africa, the desert and rivers of Mesopotamia, the desert of Palestine, or the ice-bound wastes of Russia. Equipment and transport, therefore, require revision for campaigns in the East, for our experience in the Great War showed that cases of sickness far outnumber those of wounds in Eastern theatres of war.

1 "Final Address to the Officers of the Senior Division, Staff College," December 20, 1911. Published in the *Army Review*, vol. ii, 1912.

TABLE I.—APPROXIMATE AVERAGE RATION STRENGTH OF THE FORCES ENGAGED IN THE GREAT WAR, 1914-1

Theatre of Operations	1914 (August-December).				1915.				1916.					
	British and Dominion.	Indian, African or other Natives.		Total.	British and Dominion.	Indian, African or other Natives.		Total.	British and Dominion.	Indian, African or other Natives.		Total.	British and Dominion.	Indi
		Troops.	Followers.			Troops.	Followers.			Troops.	Followers.			Troo
France	190,000	24,572	6,000	220,572	616,086	35,568	10,688	662,342	1,322,075	12,299	2,681	1,337,055	1,894,511	11
Italy	—	—	—	—	—	—	—	—	—	—	—	—	111,996	—
Macedonia	—	—	—	—	60,889	—	—	60,889 [2]	123,394	2,616	—	126,010	182,583	2
Dardanelles	—	—	—	—	163,906	3,131	1,819	168,856	—	—	—	—	—	—
Egypt and Palestine ...	—	—	—	—	89,899	17,086	—	106,985	187,926	30,541	7,455	225,922	186,549	18
Mesopotamia	2,884	8,893	2,326	14,103	6,953	17,247	7,671	31,871	48,219	66,669	50,476	165,364	83,902	101
North Russia	—	—	—	—	—	—	—	—	—	—	—	—	—	—
East Africa	Not available				Not available				32,190	*I.* 17,628 *A.* 8,456	3,575 36,801	98,590	20,760	*I.* 14 *A.* 15
West Africa	582	2,995	5,537	9,114	1,188	5,927	24,184	31,299	1,188	5,927	24,184	31,299 [3]	—	—
S. W. Africa [1]	—	—	—	—	33,000	—	15,000	48,000	—	—	—	—	—	—
United Kingdom	1,280,468	—	—	1,280,468	1,516,785	—	—	1,516,785	1,598,457	—	—	1,598,457	1,706,733	—
Total	1,473,934	36,460	13,863	1,524,257	2,488,706	78,959	59,362	2,627,027	3,313,389	144,136	125,172	3,582,697	4,187,094	164

[1] Average strength for whole campaign shown under 1915. [2] October-December. [3] January and February, 1916. [4]

TABLE I.—APPROXIMATE AVERAGE RATION STRENGTH OF THE FORCES ENGAGED IN THE GR

ember).		1915.				1916.			
other	Total.	British and Dominion.	Indian, African or other Natives.		Total.	British and Dominion.	Indian, African or other Natives.		Total.
lowers.			Troops.	Followers.			Troops.	Followers.	
,000	220,572	616,086	35,568	10,688	662,342	1,322,075	12,299	2,681	1,337,055
—	—	—	—	—	—	—	—	—	—
—	—	60,889	—	—	60,889 [2]	123,394	2,616	—	126,010
—	—	163,906	3,131	1,819	168,856	—	—	—	—
—	—	89,899	17,086	—	106,985	187,926	30,541	7,455	225,922
,326	14,103	6,953	17,247	7,671	31,871	48,219	66,669	50,476	165,364
—	—	—	—	—	—	—	—	—	—
		Not available				32,130	*I.* 17,628 *A.* 8,456	3,575 86,801	98,590
,537	9,114	1,188	5,927	24,184	31,299	1,188	5,927	24,184	31,299 [3]
—	—	33,000	—	15,000	48,000	—	—	—	—
—	1,280,468	1,516,785	—	—	1,516,785	1,598,457	—	—	1,598,457
,863	1,524,257	2,488,706	78,959	59,362	2,627,027	3,313,389	144,136	125,172	3,582,697

[1] Average strength for whole campaign shown under 1915. [2] October-December. [3] January and Februar

	1917.				1918.			
	British and Dominion.	Indian, African or other Natives.		Total.	British and Dominion.	Indian, African or other Natives.		Total.
		Troops.	Followers.			Troops.	Followers.	
55	1,894,511	11,463	*I.* 7,854 *A.* 55,051	1,968,879	1,857,026	14,500	*I.* 14,895 *A.* 102,953	1,989,374
	111,996	—	—	111,996 [4]	94,634	—	1,753	96,387
10	182,583	2,108	17,574	202,265	128,747	3,792	27,408	159,947
	—	—	—	—	—	—	—	—
22	186,549	18,890	63,329	268,768	231,090	59,366	94,871	385,327
54	83,902	101,986	121,598	307,486	107,382	140,364	163,879	411,625
	—	—	—	—	8,259	—	—	8,259
0	20,760	*I.* 14,411 *A.* 15,531	*I.* 2,831 *A.* 141,128	194,661	16,788	*I.* 6,807 *A.* 17,438	*I.* 1,799 *A.* 85,824	128,656
093	—	—	—	—	—	—	—	—
	—	—	—	—	—	—	—	—
7	1,706,733	—	—	1,706,733	1,613,953	—	—	1,613,953
7	4,187,034	164,389	409,365	4,760,788	4,057,879	242,267	493,382	4,793,528

bruary, 1916. [4] Two months only.

War Section 51

TABLE II.—PROPORTION OF SICK TO WOUNDED IN CERTAIN THEATRES OF OPERATION DURING THE GREAT WAR, 1914-18.

Theatre of war	Sick	Wounded	Proportion sick : wounds
France	3,494,165 ..	1,983,748 ...	1·8 : 1
Italy	57,172 ...	4,689 ...	12·1 : 1
Macedonia	508,999 ...	18,961 ...	26·8 : 1
Dardanelles (British only)	145,154 ...	49,190 ...	2·9 : 1
Egypt and Palestine	503,377 ...	37,193[1] ...	13·5 : 1
Mesopotamia	822,803 ...	51,388 ...	16·0 : 1
North Russia	4,877 ...	486 ...	10·0 : 1
East Africa[2]	133,321 ...	5,120 ...	26·0 : 1
South-West Africa	24,833 ...	534 ...	46·5 : 1
Total ...	5,694,701	2,151,309	2·6 : 1
South African War, 1899-1902 ...	404,126 ...	21,292 ...	18·9 : 1

[1] Indians excluded.
[2] For 1917 and 1918 only. Followers not included.

(3) FOLLOWERS.

Followers may seem to some an unimportant factor, but those who have served in the Near and Far East realize how essential they are. They are quaint people; they come from different nations and tribes, have different customs, eat different foods, and speak different languages. Usually they drifted into a force; probably the medical authorities were never informed of their arrival, and often the force for which they worked did not know how to feed or clothe them, or interpret their wants. In East Africa, for example, hillmen were sent to swampy plains and died from malaria, while lowlanders were sent to the hills and died of pneumonia; it is little wonder that occasionally a scandal threatened. The sociology of the races that provide followers for a force is an important study, and one to which we should pay more attention if our Eastern armies are to be kept up to strength without extravagant expenditure in lives and money.

TABLE III.—ADMISSIONS TO HOSPITAL FOR SICKNESS AMONG BRITISH AND DOMINION TROOPS IN THE UNITED KINGDOM 1914-18.

Year	Average Ration Strength	Admissions to Hospital	Ratio per 1,000
1914 (August-December) ...	1,280,468 ...	101,078 ...	78·93
1915	1,478,391* ...	417,137* ...	282·15
1916	1,598,457 ...	432,168 ...	270·36
1917	1,706,733 ...	459,781 ...	269·39
1918	1,613,953 ...	555,432 ...	344·14
Average ...	1,535,600 ...	393,119 ...	256·00

* British only.

Table III illustrates the strength by years of the troops serving in the United Kingdom during the Great War, and the ratio of sick admissions. It is very important to know the effect of billeting, training, rations and the military life in general on the health of the enlisted troops. The figures demonstrated how little sickness there was. In fact, the ratio of sick admissions for the past year was greater than the ratio in any year from 1914 to 1917. Even the ratio for 1918, in spite of the influenza epidemic, compares very favourably with post-war ratios. When people say that we are a C 3 race I sometimes look at these figures and think that many do not realize that a man can do useful work although he does not come up to a certain physical standard. Hutchison supports this when, in a recent paper on prognosis in the *Lancet*, he says: "Another source of error is the attaching of too much importance to physical signs and too little to the patient's general condition. This usually leads one to make too gloomy a forecast." We know that if officers at

present serving were subjected to a thorough medical examination probably few would come up to what is officially the recognized standard. Yet they do good work. This is an important point to remember when considering the problems of man-power for any great war in the future.

TABLE IV.—APPROXIMATE TOTAL BATTLE CASUALTIES IN THE GREAT WAR, 1914-18.

Theatre of War	Killed	Wounded	Missing	P. of W.	Total	Died of Wounds
France	402,715	1,983,748	60,949	175,624	2,623,036	152,023
Italy	1,246	4,689	72	272	6,279	58
Macedonia	2,583	18,961	1,285	1,287	24,116	1,468
Dardanelles	21,734	80,735	7,318	380	110,167	8,414
Egypt and Palestine	7,483	43,712	2,625	1,411	55,231	2,993
Mesopotamia	11,008	51,388	2,217	12,879	77,492	3,806
North Russia	327	486	125	45	983	33
East Africa	3,067*	9,110	1,459	477	14,113	754
West Africa	51	327	53	—	431	6
S.-W. and S. Africa	185	560	782	—	1,527	61
Aden	70	354	18	16	458	17
China	12	54	—	—	66	3
India	36	104	1	—	141	7
Somaliland	11	44	—	—	55	—
Total	450,528	2,194,272	76,904	192,391	2,914,095	169,643

* Includes 376 followers unclassified.

TABLE IVA.—APPROXIMATE TOTAL BATTLE CASUALTIES IN THE GREAT WAR, 1914-18, AND AN ANALYSIS OF WOUNDED IN FRANCE.

Theatre of war	Killed	Wounded	Missing	P. of W.	Total	Died of Wounds
All Fronts	450,528	2,194,272	76,904	192,391	2,914,095	169,643
Western Front	402,715	1,983,748	60,949	175,624	2,623,036	152,023

Analysis of Wounded in France.

Total Wounded		1,983,748
Died of wounds		152,023
Wounded returned to duty:—		
(*a*) In France	552,468	1,602,033
(*b*) From U.K. (approx.)	1,049,565	
Percentage returned to duty		80·7
Wounded evacuated from France to United Kingdom		1,213,944

TABLE IVB.—EXPENDITURE BY THE STATE IN THE PAYMENT AND ADMINISTRATION OF PENSIONS.

	£
South African War (over a period of 12 years)	5,607,070
Great War, 1914-18	665,000,000

Tables IV and IVA deal with the total battle casualties in the Great War and the number that occurred on the Western Front. As the latter represents approximately 90 per cent. of the total, my analysis of the wounded, and later on of the sick, is confined to this campaign. The table shows the numbers killed in action, died from wounds, wounded, wounded returned to duty in the theatre of operations and from the United Kingdom, and the number evacuated to this country from France.

Apart from questions of general hygiene, the great objects of the medical service are to reduce the number of deaths from wounds, to shorten the length of time in hospital and to increase the number of men returned to duty. That explains our almost unique position in a fighting force. We further the ideals of our profession by healing the sick and suffering, but we take an active part in the fighting by supplying reinforcements. Our main work is popularly associated with the seriously wounded who have to fight for their lives. Many do not realize that great surgical institutions were set up to deal with the permanently disabled, many of whom later become a justifiable expense to the State. A popular advertisement for an ex-soldiers'

association, and the total amount—£665,000,000—paid to date in pensions and expended in the administration of the Ministry of Pensions (Table IVB)—are interesting in this connexion.

From the point of view of man-power, the number of men returned to duty in the theatre of operations is all-important. Our ability to increase this number, to reduce the number of permanently disabled men, and to decrease the number of deaths from wounds depends upon two simple factors :—

(1) Arrangements to collect cases with the least possible delay.

(2) Proper provision for their reception, treatment and evacuation.

Unless we receive cases early the men may suffer from sepsis, which prolongs their stay in hospital and delays their return to duty.

The principles of medical organization in the field are simple and easily understood, and if put into practice bring the following benefits :—

(1) Increased *morale* of an Army, brought about by the knowledge among the men that when wounded they will be collected and cared for.

(2) Increased mobility of an Army owing to proper evacuation of casualties.

(3) An early return to duty of trained men who have been slightly wounded, and so assist in the concentration of superior force.

(4) Saving in money to the State.

(5) Increased confidence among relatives at home.

In practice, when a battle is anticipated, certain obstacles immediately confront us and have to be overcome. They include :—

(1) Secrecy.

(2) The estimation of the probable number of casualties.

(3) Siting of medical units.

(4) The supply of extra units, personnel and transport.

The first three of these are "G" questions.

(1) Secrecy.

For some reason, G.O.C.'s and their general staff officers are diffident of trusting the medical staff officers. It should be clearly and definitely recognized that our ability to help them, their men and the State depends upon the information they give us. During the War relationship with the General Staff depended probably more on individual effort than on recognized policy.

(2) Estimation of Casualties.

I know well the difficulties encountered by the General Staff in estimating battle casualties, and their desire neither to voice their impressions nor to state where they expect casualties to be most numerous; but they are the only people who can give an estimate. We should be given this most important information, because the success of an operation may be influenced in no small measure by our knowledge of it.

(3) Siting of Front Line Units.

We have hopes that a definite principle will be laid down on this point in the new edition of Field Service Regulations.

(4) (*a*) Extra Units and Personnel.

Field Service Regulations state that "it is the duty of the A.G.'s branch of the staff to search for and collect the wounded after battle"; with this branch rests also the responsibility of providing extra units and extra bearers. Some staff officers think that our demands for extra units are haphazard, and that we ask too much; but it is our duty to keep before them the fact that our requirements are calculated on definite factors, such as estimated casualties from battle and sickness, rate of evacuation to lines of communication and overseas hospitals, plus a small percentage for "crisis" expansion.

(*b*) EXTRA TRANSPORT.

Extra transport is supplied by "Q" branch, who are responsible for preparing it for the conveyance of sick and wounded.

Sound knowledge of the elementary principles is of the utmost importance because the medical service first sent out with a force is constituted on very definite lines and can deal with only a certain number of casualties. Failure to appreciate the need for expansion usually leads to a breakdown in the medical arrangements, disorganization, and the evacuation overseas of trained soldiers suffering from slight disabilities, with a consequent loss to the Army and expense to the State.

I offer no apology for a simple statement of these facts and of our position, for, to quote Field-Marshal Sir William Robertson again, "if troops are not in their proper place at the appointed time, or are not complete, fully equipped, and supplied in all respects as far as it is possible for them to be, the fault lies with the Staff."[1]

TABLE V.—ANALYSIS OF ADMISSIONS FOR ALL CAUSES EXCEPT WOUNDS, WESTERN FRONT, 1914-18.

Total admissions		3,494,165
Total deaths		30,006
Total returned to duty		3,260,056
(*a*) In France	2,288,678	
(*b*) From U.K.	971,378 (approx.)	
Percentage returned to duty		93·3
Total evacuated to United Kingdom		1,020,056

Table V shows only the number of admissions, deaths, etc., for all causes except wounds on the Western Front. It is well known that in the prevention of disease in an army in the field we have played a great and important part; but have our scientists gone far enough? I feel that they have not. I think that this table shows that in congratulating ourselves on our preventive medicine, we have maintained an unwarranted silence as to the value of the practice of medicine and surgery and the allied special branches.

The success of our preventive measures is limited by our knowledge, and by the application of methods of prevention. It is difficult to progress without knowledge and difficult for any serving officers to find time for research. Many advantages are derived from team work; I hope some day to see the scientists of all three Services working in unison under one roof.

Again, the other branches of the Army include in paper schemes definite medical proposals which are, nevertheless, difficult to apply when preparations for war are actually taking place. This has happened in all our campaigns, not only at the beginning but even at the end of the War, and it happens also in peace time. For instance, take the Mesopotamian campaign. Even in 1918 men were sent from the heated plains to the cold atmosphere of the Persian hills, clothed in khaki drill and with one blanket. No wonder a severe epidemic of virulent pneumonia ensued!

Again, fit and unfit men were sent to Mesopotamia from home without sun-helmets, glare-glasses or mosquito nets; in many cases it was only a matter of hours or days before they were shipped back to the United Kingdom.

In the East African campaign, according to experts, the incidence of primary, locally acquired malaria would have been greatly lessened if the troops had not been disembarked and encamped on malarious sites.

Viewed in the light of experience, the valuable regulation that the officer commanding a unit or force is responsible for the sanitation of that unit or force is not enough. Among the greatest causes of sickness in the Mesopotamian Force were

1 "Final Address to the Officers of the Senior Division, Staff College," December 20, 1911. Published in the *Army Review*, vol. ii, 1912.

diarrhœa, colitis and dysentery. Full orders were constantly issued on the preventive measures required, yet when a force attacked and captured a Turkish position it usually encamped on that position—one of the foulest spots imaginable. The admission ratio for these diseases increased and finally became greater than that for battle casualties. I do not question the G.O.C.'s decision to encamp there, but it is too much to expect that tired troops who have marched and fought, gone hungry and thirsty over miles of desert, will set about making a camp sanitary after they have obtained their objective. There is one way and one way only, and that is to hurry a fresh sanitary battalion to the site to make good the insanitary conditions. Some day this dream will be realized. Let those who scoff and cry "expense" think of Gallipoli, where at one time it was feared that 45 per cent. of the strength would be evacuated monthly. Surely if it is worth while training and transporting the flower of our manhood, it is only common sense to take every precaution to keep them fit.

TABLE VI.—CASUALTIES ON THE WESTERN FRONT, 1914-1918.

	Percentage of total casualties, excluding missing and prisoners of war.
Killed	6·85
Died of wounds	2·59
Died, other causes ...	0·51
Wounded	31·15
Sick	58·90
	100·00

TABLE VII.—WHAT THE MEDICAL SERVICE MEANT TO AN ARMY IN FRANCE.

Total wounded	1,983,748
Wounded returned to duty	1,602,033
Percentage ...	80·7
Total sick	3,494,165
Sick returned to duty	3,260,056
Percentage ...	93·3
Total sick and wounded returned to duty ...	4,862,089
i.e., a saving to the army in France of ...	16·8 armies *

* Taking 288,236 as an average strength of an army.

It is by publishing such figures as these that should lead the responsible authorities to consider whether their force is complete in every way in the fight against disease, and if not whether they might not save both men and money by making it so. Disease takes a greater toll of casualties than battle.

Times are changing and there is a tendency to forget or ignore the part we played in the fighting machine during the Great War. That tendency will grow if unchecked. It is therefore our plain duty to examine our training and teaching so that whatever happens we shall always be in a position to bring ease and comfort to the dying and suffering, to save men for the fighting forces, and to be worthy of our Corps, our Army and our Country.

Discussion.—Sir WILLIAM MACPHERSON, K.C.M.G.—I have little to say in the way of criticism, but would point out the fallacy of comparing deaths from wounds with deaths from disease as a means of judging the effect of preventive medicine in campaigns. The relative proportion of these does not depend so much on prevention of disease as on intensity of battles. On the Western front with its enormous and continuous battle casualties, the deaths from disease became more or less insignificant in comparison, whereas in East Africa and Mesopotamia, with fever and less intensive fighting, tropical diseases became the dominant factor and battle casualties insignificant. Major Mitchell referred to the preparation of the volume on statistics for the "Medical History of the War." As Editor-in-Chief I was quite

prepared to undertake the work, but the Ministry of Pensions captured the data and undertook it, only to give it up in despair, I think. When Major Mitchell then took it on for the War Office, I doubted whether he could accomplish the task with the one lady assistant in less than twenty years. But to-day he has shown me how much I miscalculated his ability to produce already some exceptionally valuable statistical points.

Lieut.-General Sir MATTHEW FELL said that Major Mitchell had produced most interesting tabulations, which should prove of great value in the study of war casualties. Major Mitchell had touched on many controversial points. As regarded his suggestion that information was sometimes withheld which was necessary for the successful functioning of the Medical Services, General Fell pointed out that this had not been his experience and that he thought that essential information involving questions of the greatest secrecy had been given more often than was supposed. He instanced the account in Sir James McGrigor's "Autobiography" of the action of the Duke of Wellington immediately before the retreat from Burgos. He also said that he understood from Sir Charles Burtchaell that he had been informed of the approximate date of the 1918 advance by the Commander-in-Chief personally, and for some time before he had been quietly making such arrangements as were possible to meet its eventualities without attracting too much attention.

Air Vice-Marshal D. MUNRO called attention to an aspect of man power not referred to in this paper, viz., the medical selection of recruits in war, and the grading of them according to the duties which they would have to perform.

He denied the statement that the British were a C 3 nation. He thought that the experiences of the last war had taught us enough to improve the methods of selection at the outbreak of another war and referred to steps which had already been taken to ensure this.

He heartily agreed with the plea for constant training of medical personnel during peace time in the duties which they would have to perform in war. He believed that the last war had taught the General Staff a great deal about the value of medical staff work, and that as a result the status of the Medical Services stood higher now than it ever had.

Major MITCHELL (in reply) assured Sir William Macpherson that he was not criticizing him with regard to the publication of the Official Volume on the Medical Statistics of the War, as he (Major Mitchell) had to refuse to prepare it after he had left the office.

In putting forward a comparison of wounds and sickness in different theatres of war his only thought was to impress once more the old fact that, wherever we were fighting, sickness caused a greater percentage of casualties than did wounds.